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# THOUGHTS ON TELEOLOGY*

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## *Introduction: Two Strategies in Interpreting Teleology*

How shall we relate teleological notions, referring events to certain of their consequences, with causal notions, referring events to certain of their antecedents ? Two hard-headed strategies are discernible in the recent literature—hard-headed because in neither one are teleological statements taken to refer to special sorts of entities such as entelechies nor to embody special sorts of explanation by means of final causes. One such strategy is to interpret teleological statements as descriptions of plastic or self-regulating behaviour, in principle explainable in ordinary causal terms. The other strategy is to construe ostensible mention of goals future to an action as referring rather to ideas of such goals prior to the action, and hence capable of figuring in its normal causal explanation.

Each of these strategies has occasionally been used in such a way as to provoke the charge of misapplication. The first has thus, for the most part, been criticised for failing to do justice to purposive action in the higher animals and man, the second for unplausibly ascribing goal-ideas to the lower animals and to non-purposive human behaviour. These criticisms, however, leave unscathed a moderate use of either strategy within well-defined limits: so long as you do not try to reduce *purpose* to self-regulative behaviour, nor attribute *ideas, wishes, or beliefs* to behaviour that is not consciously purposive, you may proceed without hindrance.

It is this moderating assumption that I shall examine in the two main sections of the present paper: (i) First, with respect to two recent interpretations of teleology in terms of self-regulative behaviour, I shall argue that they are inadequate not merely for purposive, but also for certain sorts of non-purposive behaviour, for which I will suggest the possibility of an alternative treatment. (ii) Secondly, I wish to indicate some serious logical-semantic difficulties attending purported references to beliefs about and desires for a goal even in accounting for fully purposive human action, and I want to explore a way of handling such difficulties.

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## I *The Self-regulation Strategy*

(A) *Negative Feedback.* Perhaps the most basic recent paper applying the notion of self-regulation to the interpretation of teleology is that of Rosenblueth, Wiener, and Bigelow.[1] Convinced that 'purposefulness (is) a concept necessary for the understanding of certain modes of behaviour',[2] and that its importance has been slighted as a result of the rejection of final causes, these authors propose to explain purpose, in largely engineering terms, as behaviour 'that may be interpreted as directed to the attainment of a goal, i.e. to a final condition in which the behaving object reaches a definite correlation in time or in space with respect to another object or event'.[3] In order to accomplish this explanation, the authors introduce the terms 'input', 'output', and 'feedback', as follows. 'Input' applies to events external to an object that modify the object in any manner. 'Output', on the other hand, refers to changes produced in the surroundings by the object. 'Feed-back' may be used in two different ways. It may be applied to objects (such as electrical amplifiers), some of whose output energy is returned as input. In such cases feed-back is positive in that the output re-entering the object 'has the same sign as the original input signal',[4] thus adding to this signal rather than correcting it. On the other hand, feed-back is negative when the object's behaviour is 'controlled by the margin of error at which the object stands at a given time with reference to a relatively specific goal'.[5] In such cases, 'the signals from the goal are used to restrict outputs which would otherwise go beyond the goal'.[6]

With the notion of negative feed-back at hand, the authors propose that teleological behaviour be construed as behaviour controlled by negative feed-back.[1] 'All purposive behavior', they say, 'may be considered to require negative feed-back. If a goal is to be attained, some signals from the goal are necessary at some time to direct the behavior'.[2] We may, of course, construct a machine that will impinge on a luminous object although the machine is insensitive to light, as well as to other stimuli emanating from the object. It would however, be a mistake to consider such impingement behaviour purposive inasmuch as 'there are no signals from the goal which modify the activity of the object *in the course of the behavior*'.[3]

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[1] A. Rosenblueth, N. Wiener, and J. Bigelow, 'Behaviour, Purpose and Teleology', *Philosophy of Science*, 1943, **10**, 18ff.

[2] Ibid., p. 23 [3] Ibid., p. 18 [4] Ibid., p. 19 [5] Ibid. [6] Ibid.

By contrast, some machines are 'intrinsically purposeful', for example, 'a torpedo with a target-seeking mechanism'.[4] The behaviour of such objects involves 'a continuous feed-back from the goal that modifies and guides the behaving object'.[5] The path followed by the torpedo, for example, is controlled by the signals it receives from the moving target.

The foregoing conception of teleology is one whose scientific and practical importance is generally acknowledged and is surely not in question here. That it nevertheless fails adequately to characterise clear instances of purposive behaviour is an independent point that emerges from the following considerations.

The authors interpret purposive behaviour as directed toward 'a final condition' in which the object achieves correlation with respect to some other entity. Since they describe the goal of such behaviour as something that emits signals guiding the behaving object, the goal cannot be identified with the final condition of correlation mentioned above. For one thing, this final condition, if it occurs at all, is later than the behaviour in question and cannot therefore be supposed to guide it if the notion of final causes acting upon earlier events is rejected. For another, the authors describe cases of 'undamped feed-back', in which a purposive machine, by increasingly larger oscillations, overshoots the mark and fails to attain the final condition toward which its behaviour is directed, though such behaviour has presumably been modified by signals from the goal. The goal, as described by these authors, cannot, therefore, be identified with the final condition of correlation and must rather be construed as that with which correlation is supposed to occur, i.e. what we may call 'the goal-object'. This goal-object, even where correlation fails to occur, emits signals that modify the purposive activity of the behaving object.

[1] Ibid., p. 24 [2] Ibid., p. 19 [3] Ibid., pp. 19–20, italics in original text.
[4] Ibid., p. 19 [5] Ibid., p. 20

### *The difficulty of the missing goal-object*

Now it is just the existence of such a goal-object that cannot be assumed in all cases of purposive behaviour. As pointed out in a critical paper by R. Taylor,[1] a man's purpose in groping about in the dark may be to find matches that are not there, his purpose in going to the refrigerator may be to obtain a non-existent apple, he may seek the philosopher's stone, the holy grail, the fountain of youth, or a live pulsing unicorn. In every such case his behaviour is clearly purposive and yet in none is this behaviour guided by signals emitted from a goal-object, correlation with which represents the final condition toward which the behaviour is directed. Generally, if a man's purpose is to obtain object *O*, we cannot infer the existence of something *x*, identical with *O*, such that he seeks *x*, nor can we infer, from the fact that a man's purpose is to obtain something of kind *K*, that there exists some object of kind *K* for which he strives.[2]

This criticism, it will be noted, establishes the inadequacy of the Rosenblueth-Wiener-Bigelow interpretation only with respect to purposive cases. Indeed, Taylor supposes that we have here an ' irreducible difference ' between human beings and machines,[3] suggesting thereby that what may be called ' the difficulty of the missing goal-object ' does not arise for non-purposive cases on the Rosenblueth-Wiener-Bigelow account. As against this suggestion, I now argue that the same difficulty arises also with respect to non-purposive teleological behaviour in non-human as well as human organisms, and conceivably in machines too.

Consider a standing passenger thrusting his foot outwards suddenly in order to keep his balance in a moving train, a rat depressing the lever in his experimental box in order to secure a food pellet, a small infant crying in order to attract mother's attention. To describe each of these cases as has just been done is to provide a teleological account relating the behaviour in question to some selected end. It is not, however, to attribute purpose to the organism concerned, in the sense of our previous instances, for example. If the knight's purpose in travelling far and wide is to find the holy grail, he has chosen to search for it in the hope of finding it. If a man's purpose in opening the refrigerator door is to get an apple within, he opens it with the intention of getting an apple within. It would in most cases be peculiar, however, to describe the train-rider's sudden kick as a product of choice or as an expression of intention. Psychologists, similarly, often feel uncomfortable in describing the rat as choosing to depress the lever, or as depressing it with the intention of securing a pellet. Finally, we should not generally say of a small infant, even when it cries in order to attract mother, that it has chosen to cry with the intention of attracting mother—though it may of course do the latter at a later stage, when its crying will be interpreted quite differently by its elders. I suggest these cases merely as examples of a large group for which we willingly offer teleological accounts but withhold ascriptions of intention, choice, and purpose.

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[1] R. Taylor, ' Purposeful and Non-Purposeful Behavior : A Rejoinder ', *Philosophy of Science*, 1950, 17, 329

[2] Such inferences, invalid in cases where we have an apparent reference to a goal-object, cannot even be formulated naturally for purposive cases lacking such reference. The mystic striving to become more humble through spiritual training is not moving toward correlation with some object, guided by signals emanating from it.

[3] Op. cit., p. 330

It does not take much reflection to see that the difficulty of the missing goal-object arises even for the moderate attempt to apply the Rosenblueth-Wiener-Bigelow interpretation to this set of non-purposive cases. For we cannot plausibly suppose our train-rider to be receiving guiding signals from some region with which his foot is to be correlated. Neither, when the psychologist stops replacing the rat's pellets, can we describe the rat as receiving directive signals from some such pellet. Nor, finally, when mother, expecting baby to sleep, steps out to the corner store, is she available for the issuing of signals guiding the infant's behaviour toward final correlation with herself. The Rosenblueth-Wiener-Bigelow analysis cannot therefore be judged generally adequate even for non-purposive teleological behaviour, nor does it seem to suffice even when we restrict ourselves to non-human cases.

## *Teleology and learning*

A suggestion: For at least *some* non-purposive cases, I suggest that an interpretation in terms of learning may throw some light on teleological description, where analyses in terms of self-regulation fail. In the case of the infant, the suggestion is that our ' in order to '-description of its present crying reflects our belief that this crying has been learned as a result of the consequences of like behaviour in the past—more particularly, as a result of having received mother's attention. Having initially cried as a result of internal conditions *C*, and having thereby succeeded in attaining motherly solace, representing a type of rewarding effect $E$, the infant now cries in the absence of $C$, and as a result of several past learning sequences of $C$ followed by $E$. The infant's crying has thus been divorced from its original conditions through the operation of certain of its past effects.[1] These past effects, though following their respective crying intervals, nonetheless precede the present crying interval which they help to explain. The apparent future-reference of a teleological description of this present interval is thus not to be confused with prediction, nor even with mention of particular objects in the current environment, toward which the behaviour is directed. Rather, the teleological statement tells us something of the genesis of the present crying, and in particular, of the prominent rôle played by certain past consequences in this genesis. Such an account is perfectly compatible with normal causal explanation, and it indicates why goal-objects may very well be missing in some cases for which teleological description is appropriate. It shows also that we have in this respect no irreducible difference between human beings and machines, since machines capable of learning through the effects of their own operations are equally subject to such teleological description.

(B) *Plasticity of Behaviour.* The interpretation of teleology proposed by R. B. Braithwaite,[2] though it resembles the Rosenblueth-Wiener-Bigelow approach, is considerably different in detail and restricts itself explicitly to what Braithwaite calls ' goal-directed behaviour ' as distinct from ' goal-intended behaviour ',[3] thus avoiding criticisms such as that of Taylor, based on purposive cases. Braithwaite's use of the self-regulation strategy is deliberately moderate; he wants to interpret 'all teleological explanations which are not reducible to explanations in terms of a conscious intention to attain the goal'.[4]

That the notion of *causal chains* is fundamental in the analysis of teleology is, for Braithwaite, shown by the fact that the organism's behaviour does not directly produce the goal; it constitutes, rather, part of a causal sequence of events progressing toward the goal. What, however, distinguishes teleological causal chains from all others with which science is concerned ? Here Braithwaite follows E. S. Russell in proposing as the main clue ' the active persistence of directive activity towards its goal, the use of alternative means towards the same end, the achievement of results in the face of difficulties '.[1] Such plasticity of behaviour is ' not in general a property of one teleological causal chain alone: it is a property of the organism with respect to a certain goal, namely that the organism can attain the same goal under different circumstances by alternative forms of activity making use frequently of different causal chains '.[2] To specify this plasticity further, Braithwaite provides a schematic description.

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[1] R. S. Peters, ' Symposium : Motives and Causes ', *Arist. Soc. Suppl.*, 1952, **26**

[2] R. B. Braithwaite, *Scientific Explanation*, 1953, pp. 319ff

[3] Ibid., p. 325

[4] Ibid.

We have, let us suppose, some object *b* which, in the normal case, is to be taken as a physical or organic system. Every event at any time in *b* is assumed determined by the whole preceding state of *b* taken together with the set of actual field or environmental conditions causally relevant to this event. Consider now a causal chain *c*, comprised of events in the system *b* during a particular interval and following immediately upon the initial state *e* of the system. Suppose also that the set of actual field conditions causally relevant to the events comprising the chain is *f*. Then *c* is uniquely determined by the initial state *e* taken together with the set of field conditions *f*.

Causal chains may now be related to a goal gamma as follows. Every chain ending in a gamma-event without containing any other such event is a gamma-goal-attaining chain. Relative to a given initial state *e* of *b*, we may consider the class of all (possible) sets of field conditions *f* uniquely determining chains that are gamma-goal-attaining. This class is the variancy Phi, with respect to *b*, *e*, and gamma, comprising the ' range of circumstances under which the system attains the goal '.[3]

Plasticity may be attributed to the system when the variancy has more than one member, so that the goal may be attained under alternative environmental circumstances, though not necessarily by means of alternative causal chains. Teleological explanations assert that the behaviour in question is plastic, and are intellectually valuable to the extent that this plasticity is not asserted on the basis of known causal laws of the system's mechanism, but rather on the basis of past observation of the conditions under which similar behaviour has taken place. Further, teleological explanations predict the occurrence of some gamma-goal-attaining chain on the basis of knowledge of the system's plasticity; they predict, in other words, that the set of field conditions that will in fact occur is a member of the appropriate variancy Phi, on the ground that Phi includes the class Psi of all sets of field conditions likely to occur.

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[1] Quoted from E. S. Russell, *The Directiveness of Organic Activities*, p. 144, by Braithwaite, op. cit., p. 329

[2] Braithwaite, op. cit., p. 329

[3] Ibid., p. 330 : I do not discuss at length the problem of determining the *beginnings* of goal-attaining chains, on this view. Strictly, according to the definition given, a cat's capture of its first mouse ends a single mouse-attaining chain that began with the birth of the cat.

It is at once apparent that, since Braithwaite does not construe goal-direction in terms of a relation to some goal-object but rather in terms of the variability of appropriate field circumstances, the difficulty of the missing goal-object does not arise in his scheme. Fido's pawing at the door need not be controlled by signals from outside; it need only lead to his being let out under alternative circumstances, say when his master is at home or when only his master's young son is at home. The rat need not, as he depresses the lever, be receiving any stimuli from the pellet that drops into his cup only after the lever is depressed; it is enough that his depressing of the lever results in his obtaining the pellet under a variety of experimental conditions.

### *The Difficulty of Goal-failure*

Braithwaite's interpretation is, however, subject to other difficulties. For he appears to hold that particular teleological explanations do not merely ascribe plasticity to the behaviour being explained but also predict goal-attainment in the circumstances that will in fact ensue. It is perhaps the least of the difficulties of this view that its use of the term ' explanation ' is strange, bearing little relation to normal causal explanation. Of crucial importance, however, is the fact that teleological descriptions, whether or not they qualify for the title ' explanation ', do not generally predict the attainment of the goal indicated. It is interesting that, despite the requirement of Rosenblueth, Wiener, and Bigelow for a *goal-object*, they allow for non-attainment or *goal-failure* in their examples of undamped feed-back resulting in overshooting the target. But it is easy to multiply other examples of goal-failure. If Fido, trapped in a cave-in, is in fact never reached, is it therefore false that he pawed at the door in order to be let out ? If, as in the case previously described, the psychologist stops replacing the consumed pellets with new ones, is it false to say the rat continues to depress the lever in order to obtain a pellet ? To suppose, with Braithwaite, that all teleological explanations carry with them an ' inference that the set of relevant conditions that will in fact occur in the future will fall within the variancy ' [1] is to answer the preceding questions affirmatively and quite unplausibly.

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[1] Ibid., p. 334

The difficulty of goal-failure may, of course, be avoided if the prediction of goal-attainment is eliminated from the content of teleological descriptions. Retaining the rest of Braithwaite's scheme, we should then take such descriptions simply as attributing plasticity to the behaviour in question with respect to the goal indicated. We should be saying of the system *b* exhibiting the state *e* that there is more than one possible set of field conditions *f*, such that, were *f* conjoined with *e*, some suitable goal-attaining chain would ensue. We should not, however, be predicting that one of these sets will in fact be presently realised, and our statement would therefore be protected from falsification through goal-failure.

### *The Difficulty of Multiple Goals*

This proposal is, however, immediately confronted with a new difficulty that we may call ' the difficulty of multiple goals ' ; the proposal becomes too inclusive to differentiate between acceptable and inacceptable teleological descriptions in an indefinitely large number of cases. These cases may be indicated schematically in Braithwaite's terms. Imagine that for the system *b*, and relative to the present state *e* of this system, the class of those sets of field conditions *f* uniquely determining gamma-goal-attaining chains has more than one member. The system in state *e* is thus plastic with respect to gamma. Suppose, now, that the class of those sets of field conditions *f* uniquely determining delta-goal-attaining chains relative to *b* and *e* also has more than one member. The same system in state *e* is thus also plastic with respect to delta. If our teleological description of the system's present behaviour embodies neither a prediction of the attainment of gamma nor a prediction of the attainment of delta but restricts itself merely to an assertion of the plasticity of this behaviour, we should be able, with equal warrant, to frame our teleological description either relative to gamma or relative to delta. We should, that is, be able to say either that *b* exhibits *e* in order to attain gamma or that *b* exhibits *e* in order to attain delta. This is, however, exactly what we cannot generally say.

The cat crouching before the vacant mouse-hole is crouching there in order to catch a mouse. Since no mouse is present, there will in fact be no goal-attainment. Nevertheless, the cat's behaviour is plastic since there are various hypothetical sets of field conditions, each set including one condition positing a mouse within the cat's range, such that, in conjunction with the cat's present behaviour, each set determines a mouse-attaining causal chain. On the other hand, there are also various other hypothetical sets of field conditions, each set including one positing a bowl of cream within the cat's range, such that, conjoined to the cat's present behaviour, each set determines a cream-attaining causal chain. It should therefore be a matter of complete indifference, so far as the present proposal is concerned, whether we describe the cat as crouching before the mouse-hole in order to catch a mouse or as crouching before the mouse-hole in order to get some cream. The fact that we reject the latter teleological description while accepting the former is a fact that the present proposal cannot explain.

A limiting case of this difficulty occurs when the state *e* is one for which we should reject *every* positive teleological description. Imagine, for example, that *e* is the state of physical exhaustion and that the relevant class of field-condition sets is the one just considered in connection with mouse-attainment, suitably supplemented by conditions stipulating the cat's recuperation and assumption of a crouching position before the mouse-hole. The present proposal would then warrant us in saying, absurdly, that the cat is physically exhausted in order to catch a mouse.

Other examples of the difficulty of multiple goals arise in cases where every gamma-goal-attaining chain is also a delta-goal-attaining chain or contains such a chain as a part, so that if the variancy with respect to gamma has more than one member, so has the variancy with respect to delta. Consider, for example, an infant crying in order to get mother's cuddling, which is always preceded by the sound of mother's footsteps. Since every set of field conditions determining a cuddling chain in this situation also determines a footstep chain, and since we may assume the class of such sets determining cuddling chains to have more than one member, we may infer that the class of sets determining footstep chains also has more than one member. Thus we should again be warranted by the present proposal in saying either that the infant cries in order to receive its mother's cuddling or that it cries in order to hear mother's footsteps. The proposal again proves itself too inclusive to differentiate between teleological descriptions we accept and those we reject.

### *The Learning Suggestion again : a Caution*

Can the wanted distinctions be made on the basis of the learning interpretation suggested earlier ? The answer seems to be ' yes ' with regard to the examples we have considered. Thus we do not believe the cat has learned to crouch before the mouse-hole as a result of having been rewarded with a bowl of cream for having done so in the past. Nor do we suppose the infant's crying to have been learned as a result of having heard its mother's footsteps following past crying intervals. Nevertheless, I *surely do not* wish to propose the learning interpretation as an adequate analysis simply on the basis of a few examples and I am convinced, moreover, that several lines of interpretation will be required to account for the variety of statements commonly labelled ' teleological '. Furthermore, to appeal to our beliefs as to how the behaviour in question has been learned is, I think, to provide no firm philosophical answer : it is rather to suggest the importance of addressing our critical attention to the field of learning.

## 2 *The Goal-Idea Strategy*

### *Background and Formulation*

The second strategy mentioned at the outset consists, it will be recalled, in replacing reference to *goals* future to an action by reference to prior *ideas of such goals* functioning as causes of the action. What is the motive for this strategy ? Suppose we explain John's decision to take a pre-medical course by saying that he decided to do so in order to enter medical school. Of what explanatory value is the apparent reference to John's future entrance ? If it indeed comes to pass, it follows his present curricular decision and so cannot be among its causes. On the other hand, if it never comes to pass—for example, if John fails to complete his pre-medical training, we surely cannot count such entrance as a cause or partial cause of his actual decision. If the brick in fact did *not* strike the window, we cannot well say the window shattered because of the impact of the brick. What sense shall we then make of the fact that, although John's entrance to medical school is possibly fictional and, in any event, future to his decision, he clearly did choose his pre-medical course in order to enter medical school at a later date ? The natural, and quite plausible, suggestion is to say that John's prior *desire* to enter, and his prior *belief* that entrance is contingent on choice of a pre-medical course, jointly determine his choice. Even if his desire is in the end thwarted and his belief in fact false, they are nevertheless realities of the situation preceding his choice, and they contribute, moreover, to its causal determination.

This strategy, quite plausible in the case just considered, has been rightly criticised in numerous others for projecting belief and desire into phenomena without adequate justification. As Ducasse has put it,

> the disrepute into which teleological explanations have fallen is doubtless due to their having been so frequently thus put forth in cases where the existence of the agent appealed to and of his beliefs and desires was not already known, but invented outright and purely *ad hoc* . . . But when antecedent evidence for their existence is present (e.g., when the hypothetical agent is a human being), a teleological explanation is methodologically quite respectable, although, like any other, it may in a given case not happen to be the correct one.[1]

Accordingly, Ducasse suggests the following as essential elements in genuine cases of purpose, rendering teleological explanation possible in the form exemplified by our recent illustration :

'1. *Belief* by the performer of the act in a law . . . e.g. that if *X* occurs, *Y* occurs [or that *Y* is contingent upon *X*].
2. *Desire* by the performer that *Y* shall occur.
3. *Causation by that desire and that belief jointly*, of the performance of *X*.' [2]

The sort of rule allowing explanation of the occurrence of *X* is further suggested by Ducasse as ' If an agent believes that *Y* is contingent upon *X* and desires *Y*, then that agent is likely to do *X*.' [3]

Now, even if we restrict the present strategy to clearly purposive human action in the sense specified by Ducasse, for example, we face serious problems in formulating the required explanatory statements. By way of illustration, let us try to explain John's curricular choice according to some such schema as that suggested by Ducasse. We ask, ' Why does John choose a pre-medical course ? ', and we receive the following statements in reply :

(i) John desires John's entrance to medical school.
(ii) John believes that John's entrance to medical school is contingent on John's choice of a pre-medical course.
(iii) Whenever someone desires something, believing that it is contingent on something else, he performs this other thing.

In symbols :

$$(x)\,(y)\,(z)\,((x \text{ desires } y \,.\, x \text{ believes that } y \text{ contingent on } z) \supset x \text{ performs } z),$$

(with the range of the variable ' z ' restricted to acts or choices).

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[1] C. J. Ducasse, ' Explanation, Mechanism, and Teleology ', *Journal of Philosophy*, 1926, 23. Reprinted in Feigl and Sellars, *Readings in Philosophical Analysis*, New York, 1949, pp. 543-544.

[2] Ibid., p. 543. Addition in brackets mine; This seems quite in line with Ducasse's intent.

[3] Ibid.

This sample explanation is obviously crude and oversimplified, raising numerous questions peripheral to the logico-semantic difficulties that are my present concern. Thus, for example, to say ' John performs John's choice of a pre-medical course ', in concluding our explanatory argument here, is indeed a violation of English but one that provides a uniform technique for describing action. More important is the fact that (iii) requires much qualification before it can be taken seriously as a truth of conduct. Nobody can perform every act upon which the fulfilment of his desires is believed to be contingent, for some such acts are beyond his power. Nor does anyone actually choose to do everything within his power that he believes necessary for the attainment of every one of his desires, even if the dictates of the latter are compatible. Nevertheless, if John's desire and belief are to figure in the causal account of his choice, as the strategy we are considering requires, some generalisation incorporating these three elements but considerably more complicated than (iii) must be supposed as a premise. Since any such generalisation faces the same difficulties that I wish to indicate with reference to (iii), the latter statement may here be taken as a simple model for a general problem.

### *Logico-ontological Difficulties : Objects of Desire*

We may begin by asking, 'What is the intended range of the variable 'y' in (iii)?' Surely this range is not limited to actual things for, as mentioned earlier, the argument is intended to hold even if John's entrance to medical school never comes to pass. As in the case of purpose discussed earlier, we cannot infer existence of the objects of desire. To expand the range of ' y ' to include possible but non-actual entities is not, moreover, sufficient, for the squaring of the circle and other impossibles have undoubtedly been desired. Allowing the variable ' y ' to range over impossible so-called things as well is a step not many are prepared to take. It is bad enough to have to say, ' There is a possible but non-actual pot of gold at the end of the rainbow which has been desired by men ', but to say, ' There exists a non-actual and, moreover, impossible squaring of the circle which has been desired by many ' puts an impossible strain on the word ' exists ', to say the very least.

### *Logico-ontological Difficulties : Belief*

The variable ' y ', further more, appears not only in the context ' $x$ desires $y$ ', but also in the context ' $x$ believes that y contingent on $z$ ', and both ' *y* ' and ' *z* ' refer back to prefixed quantifiers. Such quantification into belief contexts is, as Quine has repeatedly warned, a problematic procedure.[1] The occurrence of singular terms within belief contexts is, as he puts it, not purely referential in that the truth of such contexts taken as wholes depends not merely on the objects named by such terms but also on the manner of naming ; belief contexts are, in short, referentially opaque. To use Quine's example, Philip may believe that Cicero denounced Catiline without believing that Tully denounced Catiline, though Cicero and Tully are identical. To replace either the name ' Cicero ' or the name ' Tully ' with a variable ranging over concrete objects such as people, in quantified belief sentences about Philip, thus yields an unintelligible result, since Philip's belief cannot be interpreted as a relation between Philip, Catiline, and some man to whom the names ' Cicero ' and ' Tully ' equally apply.

Quine has indeed admitted with reference to modal contexts, which are also referentially opaque, that a suitable limitation of the ontology to objects not nameable by non-synonymous names renders unrestricted quantification legitimate once more.[2] This result is due to the fact that interchangeability of synonymous names preserves truth value even within modal contexts, thus rendering the manner of naming again irrelevant and allowing modal statements to be true or false depending simply on the objects referred to. If, however, as I have elsewhere argued,[3] synonymy is not sufficient to guarantee interchangeability with preservation of truth value within belief contexts, the ontological restriction above mentioned will not legitimise quantification into *belief* contexts despite its adequacy for *modal* contexts. To be safe, we should need to restrict our ontology to objects not nameable by non-identical names within our language. Instead of the concrete object Cicero, for example, we would now recognise as many objects in our new ontology as there are different names or descriptions of Cicero, whether synonymous or not. This seems a desperate expedient indeed, not to say a transparent projection of our language into the world of things.

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[1] W. V. Quine, ' Notes on Existence and Necessity ', *Journal of Philosophy*, 1943, **40**, pp. 113ff.; Chap. VIII of *From a Logical Point of View*, Cambridge, Massachusetts, 1953; and ' Quantifiers and Propositional Attitudes ', *Journal of Philosophy*, 1956, **53**, pp. 177ff.

[2] W. V. Quine, *From a Logical Point of View*, pp. 150ff.

[3] I. Scheffler, ' On Synonymy and Indirect Discourse ', *Philosophy of Science*, 1955, **22**, pp. 39ff

Notice, finally, that the variable '$z$' occurs once within the belief context and once outside it, subject to the same external quantifier, thus serving to tie together the agent's act with his view of his act, as the variable '$y$' serves to tie together the agent's desire and his beliefs about the conditions of fulfilment of that desire. Waiving all questions as to the nature and individuation of such entities as acts, it should be noted that these connections between act, desire, and belief are essential not only to the strategy under consideration, but also to every developed conception of rational behaviour. Indeed, if we have no way of formulating these connections in such a way as to avoid the difficulties discussed, we lack a satisfactory account of human action. The following remarks are devoted to the exploration of a possible solution.

### *Suggestion for an inscriptional interpretation*

Suppose we try to construe desire, belief, and performance, at least with respect to the present schema, as relations between agents and inscriptions. The initial advantage to be hoped for is, of course, the relative clarity of inscriptions as concrete physical objects in comparison with the possibles, impossibles, and other strange entities posited by the approaches we have so far considered. The existence of inscriptions is, furthermore, completely independent of the existence of their purported objects. Though no pot of gold dangles from the end of any rainbow, we cannot infer that no inscription affirms that there is such a pot of gold. Where an object of desire fails to exist, its purported description need not, correspondingly, fail to exist. Even where something lacks not only being but possibility, its non-existence does not generally carry over to its purported description. To assert the existence of something both square and not square is a serious and offensive thing to do ; to attribute being to some inscription asserting such existence is something else and quite innocuous. Can we reconstrue the explanatory argument (i)-(iii) in such a way as to capitalise on the advantages of an inscriptional formulation ?

### *Desiring True*

Suppose we begin by transforming (i) into the following statement, converting the singular term naming the object of John's desire into a clause :

(I) John desires that John enter medical school.

Let us now construe this statement as :

(i') John desires-true some that-John-enters-medical-school inscription.

*Desiring-true* is here to be taken as a relation between people or other suitable agents on the one hand and inscriptions on the other, and does not require that the agent produce or even understand the inscription that he desires-true. To interpret this first premise of the explanatory argument as we have done can, moreover, be no more obscure than to construe it as relating John to some such entity as a hypothetical state of affairs named by the that-clause of the premise. For it is always possible to satisfy proponents of the latter construal by explaining the desiring-true of a given inscription as the desiring of that state-of-affairs which is named by it or by the proposition it expresses. The present interpretation, it should also be noted, treats the that-clause of (I) as a single predicate that applies to inscriptions peculiarly related to itself—inscriptions constituting rephrasals of its own sentential content, *including this very content as well.*[1] Thus (i') tells us that John desires-true something which is a that-John-enters-medical-school. Every such thing, further, is an inscription rephrasing the ' John enters medical school '-inscription that forms the content of the that-clause predicate of (i'). We shall, finally, make the assumption that to desire-true any given inscription denoted by a that-clause predicate is to desire-true every such inscription.

### *Believing True*

Turning now to the second premise (ii) of our explanatory argument, let us first transform it into the following statement, eliminating reference to contingency in favour of a conditional of the type suggested by Ducasse :

(II) John believes that if John enters medical school then John chooses a pre-medical course.

Let us now interpret this statement as :

(ii') John believes-true some that-if-John-enters-medical-school-then-John-chooses-a-pre-medical-course inscription.

*Believing-true* [2] is, analogously with desiring-true, here taken to be a relation between agents and inscriptions, not implying that the agent produce or understand those inscriptions he believes-true. Such an interpretation is at least as clear as the usual construal of belief as a relation between a person and a proposition, for the believing-true of a given inscription is explicable to the intensionalist as the believing of the proposition expressed by it or by the statement it designates. The that-clause of (II) is to be treated also as a single unanalysable predicate applicable to rephrasals of its own sentential content, *inclusive of such content.*[1] Finally, we are to assume that to believe-true some inscription denoted by a given that-clause predicate is to believe-true every inscription denoted by that predicate.

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[1] See I. Scheffler, ' An Inscriptional Approach to Indirect Quotation ', *Analysis*, 1957, **14**, pp. 83ff., for a fuller explanation of this general approach. The fact that the sentential content of the that-clause predicate is included in its application guarantees the existence of an appropriate inscription by the very existence of the stated premise.

[2] This relation is suggested by W. V. Quine, ' Quantifiers and Propositional Attitudes ', p. 186. Quine uses believing-true as a relation between agents and sentences, and thus expands it from a dyadic to a triadic relation, relativising it to language.

### *Making True*

Turning now to the third premise required to complete our explanatory argument, let us replace the original (iii) with the following :

(iii') Whenever anyone $x$ desires-true any inscription $v$ and believes-true another inscription $z$, such that $z$ is the conditional of $v$ and a third inscription $w$, then $x$ makes-true $w$. (Here the range of the variable ' $w$ ' is restricted to English sentence-inscriptions of the form ' $x$ chooses. . .'.)[2]

*Making-true*, similarly to desiring-true and believing-true, is a relation between agents and inscriptions and does not require the agent to produce or even to understand the inscriptions he makes-true. Making-true a given inscription can always, furthermore, be explained as performing the act described by the inscription or by its associated proposition ; in the present case, John's making-true of a 'John chooses a pre-medical course '-inscription is explicable as his performing of the choice described, as his doing of the act. We shall, further, suppose that to make-true a given inscription is to make-true every rephrasal of it. It should especially be noted that, unlike inscriptions desired-true or believed-true, every inscription made-true is in fact true, together with all its rephrasals.

### *Reformulation of the Teleological Argument*

How shall we now explain John's choice of a pre-medical course with the aid of our reformulated premises ? From (i') we learn that

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[1] As with desiring-true, the existence of an appropriate inscription is guaranteed by the existence of the whole premise.

[2] Strictly, other restrictions on this range would be needed to prevent awkward results. It might be best, for example, to restrict the vocabulary and syntax of such inscriptions in certain ways. But I am concentrating on the general idea here and so do not enter into details.

John desires-true some inscription $x$ to which the that-clause predicate of (i') is applicable. This inscription $x$, we know, is a rephrasal of the content of this predicate itself. (ii') conveys that John believes-true some inscription $y$ denoted by the that-clause predicate of (ii'). Looking at this latter predicate of (ii'), we note that its sentential content is a conditional of which the antecedent is a *replica* of the content of the that-clause predicate of (i'). On the assumption that (i') and (ii') belong to the same language and contain no indicator terms, this antecedent must therefore also be a *rephrasal* of the that-clause content of (i'). If so, it must also be denoted by the that-clause predicate of (i'), that is, this antecedent must also be a that-John-enters-medical-school. Since, moreover, (i') says that John desires-true *some* inscription $x$ which is a that-John-enters-medical-school, he must also desire-true the antecedent in question. This follows because of our assumption that to desire-true any inscription denoted by a given that-clause predicate is to desire-true every such inscription.

Now $y$, which John believes-true according to (ii'), is denoted by its that-clause predicate. But we know that the content of this predicate itself is also so denoted. Thus we may conclude that John believes-true this very content itself, i.e. the conditional inscription of (ii'). This conclusion, it will be recalled, is warranted by our assumption that to believe-true any inscription denoted by a given that-clause predicate is to believe-true every inscription denoted by that predicate.

The conditional inscription of (ii') believed-true by John is, however, the identical one of which we already know that John desires-true the antecedent. This conditional may thus be taken as the conditional $z$ of (iii'), its antecedent may be taken as $v$ in (iii'), and its consequent may then be taken as $w$ of (iii'). The conclusion may now be drawn that John makes-true the consequent of the conditional in question as well as every one of its rephrasals. Since the consequent is made-true, it is in fact true, together with every one of its rephrasals. We may now, in particular, write down a replica of this consequent in the same language as our three premises. On the assumption that it contains no indicators, it will then also be a rephrasal of the consequent, hence true. It will thus represent the needed conclusion of our argument :

(iv') John chooses a pre-medical course.

The foregoing analysis is only exploratory. It remains to be seen to what extent the treatment of our example can be generalised. If it proves sufficiently general, it will, by avoiding reference to desired states-of-affairs, believed propositions, and performed acts, have managed also to escape the semantical difficulties plaguing the second strategy we have considered. It does not, however, provide an 'operational' definition of 'belief', 'desire', and 'performance' idioms ; thus believing-true, desiring-true, and making-true are, for all that we have said, no clearer *in application* than these original idioms themselves. Their explication presents an important problem left unsolved by our above treatment, which has addressed itself to specific semantic difficulties.

## *Summary*

1. Two strategies for interpreting teleology have been examined : the self-regulation strategy, and the goal-idea strategy. Criticism has been directed explicitly against the moderate use of each strategy, i.e. the use of the first solely for non-purposive behaviour and the use of the second solely for purposive behaviour.

2. Two basic variants of the self-regulation strategy have been criticised as inadequate to account for non-purposive (let alone purposive) teleological behaviour. (A) The inadequacy of the 'negative feedback' variant has been discussed in connection with the difficulty of the missing goal-object. (B) The inadequacy of the 'plasticity' variant has been argued by reference to the difficulty of goal-failure and the difficulty of multiple goals. (C) These inadequacies are not, of course, taken as proving that the self-regulation strategy is never applicable, but only as showing that it is not sufficient, even for non-purposive teleological behaviour.

3. With respect to non-purposive cases, another interpretation of teleology, in terms of learning, is suggested. (A) This interpretation construes the future-reference of teleological statements not as predictive nor as descriptive of objects in the current environment, but as pointing to the rôle of consequences in the genesis of the learned behaviour in question. (B) The learning interpretation seems to avoid the difficulties previously discussed, but the matter both requires and deserves further study. (C) The learning interpretation is, in any event, proposed as only *one* among several that are likely to be needed.

4. The second (i.e. goal-idea) strategy has been argued to involve logico-ontological difficulties in its natural formulation, which makes use of the notions of belief, desire, and performance. These difficulties hold even when the strategy is applied to clearly purposive cases.

5. An inscriptional proposal for avoiding these difficulties is explored. (A) This proposal makes use of the new notions ‘ desiring-true ’, ‘ believing-true ’, and ‘ making-true ’, holding between agents and inscriptions. (B) A sample teleological explanation is reformulated in terms of these notions, to show how the argument may be carried through in a way that is both materially adequate and free of the logico-ontological difficulties mentioned earlier. (C) This proposal, too, requires further study, and is, in any event, not intended to give an *explication* or operational analysis of belief, desire, and performance.

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# TIME AND THOUGHT*

## G. L. S. Shackle

### 1 *Two Views of Time*

Are there, I would first of all ask, two utterly different views of time, the outside view and the inside view ? What I mean by the outside view is illustrated especially by the ways of thought of academic people in their academic capacity. The mathematician and the historian are the examples I have particularly in mind. The mathematician treats time as a space, or as one dimension of a space, in which all points have an equal status or importance or validity *together*, within one and the same prospect of the world; they have, as I would paradoxically say, a *simultaneous* validity, each of them means the same to him when he thinks about them all in one thought. This attitude of the mathematician is brought to a focus when we consider what is implied by his writing down a differential equation to express, say, the motion of the ' particle ' of classical dynamics. In this equation, which, at each moment when he contemplates it, he regards as valid, he has implicitly

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[1] This paper stands as it was written in the winter of 1957-58, as one more result of the kindness and encouragement of Professor Burton Keirstead. Since completing it, I have had the marvellous intellectual experience of reading Professor Michael Polanyi's *Personal Knowledge*. This book, whose passages of luminous beauty give it rank as literature no less than as science, provides an epistemic setting within which, perhaps, the difficulties that I have tried in this paper to define might find solutions and lead to constructive development. To Professor Karl Popper I owe an early impulse to attack these questions. The excitement and ferment of thought that I carried away from a lecture of his in 1936 live with me still. He has now with very great kindness written out a detailed criticism of the views here expressed, and some radical differences between us will act as a fresh incentive to thought. Professor Keirstead himself takes issue with me in a forthcoming article in *Metroeconomica*, with delicacy and subtlety that are, perhaps, too kind. Amongst such relevant work as I have read in earlier years Professor W. B. Gallie's masterly book on Charles Peirce is outstanding. Mr J. W. N. Watkins by his perfect understanding of what I was driving at in earlier work, and now Professor Johan Åkerman in a just-published article of searching insight in *Kyklos*, have greatly improved my grasp of my own ideas. To each of them I am very deeply grateful. All my efforts for eight or nine years past have drawn strength from Professor C. F. Carter's critical support.

stated the position which the particle will, or did, or would, occupy at each instant during some stretch of time. The equation cannot exist and have meaning, therefore, unless all those instants can be looked on as having an equal validity *together.* In the mathematician's act of writing down his equation, all the instants and the corresponding positions of the particle are folded up into one indivisible statement. At which of these instants does he place himself, at which does he take his stand ? At none of them, he is an *outside observer,* not part of the system he is describing, and for him all the instants are, in the instantaneous logic of his own thought, equally and simultaneously valid and meaningful.

Consider the historian who is thinking, say, about the constitutional changes produced in England by the war between king and parliament in the seventeenth century. Can he at that moment help looking back, half unconsciously perhaps, at the beginnings of political freedom in England, at Magna Carta, at Simon de Montfort; and looking ' forward ' to the great reforms of the nineteenth century, and on to the beginnings of the Welfare State in Asquith's government of 1911 ? All this long process presents itself to him in one panorama, as a unity, every part of it as real as every other part; he is an outside observer, not himself part of what he describes.

With this outside, detached, sophisticated view of time, I want to contrast the *inside* view which each of us has in the very act of living, the time *in* which we sense-perceive, feel, think, imagine, and decide. From this inside view, the time of our actual psychic experience is but a moment, utterly solitary in its isolation from all other moments. It is what I would like to call the *solitary present* or the *moment-in-being.* The experienced moment, the moment-in-being, is for the individual person the only thing there is, the only actuality. I do not mean that it is the only *real* thing: he may believe in the reality of things outside his own sense perceptions and psychic processes. Let me put the contrast, as I see it, between the outside and the inside view of time in another way. Time from the inside is the time *in* which we think, time from the outside is the time about which we think. Thus I want to attach a rather special meaning to the word *actual,* the meaning of presence and pressure upon the mind, the fleshly existence of events, as distinct from the content of those events that pass upon the mental screen of memory and imagination. I would say, then, that for any one person, no two distinct moments can be actual together, the actuality of one denies and excludes the actuality of any other.

In what sense, then, can the *actual* experiences of two distinct moments be compared? In no sense at all, such comparison is meaningless and impossible, for there is no ' common ground ', no meeting place for two mutually destructive actualities. Within the actual present, of course, different elements of the picture can be compared. These elements include, on the one hand, the sense-impressions of what is going on ' now ' and also, I would say, that sort of neurological expectation, that forward-feeling of our nervous system which links one moment to the next; and on the other hand, those things which the mind creates, as it were, unaided: memories and *experiences by imagination.* Memory and imagination are both of them part of a person's present experience, they belong to the essence of the moment-in-being, they are in it and of it. The content or subject-matter of their images does, indeed, bear a label with a date other than that of the moment-in-being, but this fact no more allows us to treat these images as effective *substitutes* for the actuality of other moments than the presence in a painting of the images of other places enables us to treat the painting as a full substitute for those other places.

## 2 *Dynamic and Imaginary Movement in Time*

The moment-in-being must surely be looked on as something which exists only by changing. It is a unity, a self-interlocking whole, yet I suggest we can better use the analogy of a wave rather than that of a particle. Can a wave be identified? There is, presumably, a certain balance and mutual dependence between the parts of a wave, the crest and the trough, which entitle and require it to be thought of as a unity, yet we can ' cut off ' the segment at any two suitably related points, at the bottoms of two troughs or at the tops of two crests. Any such analogy is crude in the extreme, perhaps scarcely worth invoking. Yet it seems vital to distinguish between two ways in which the moment-in-being, time from the inside, is related to the calendar axis, time from the outside.

By the phrase *dynamic movement in time* I refer to the notion, first, that the moment-in-being, even from the outside view, is not a bead upon a string but rather a colour of a spectrum, distinguishable as something in itself yet overlapping and growing out of and into other colours, so that while it is possible to name a particular wave-length of light, say 0.0000 metres, yet such a wave-length is not the whole of a discernible colour but rather corresponds to an *instant* or point of time than a moment; and secondly, that the moment is the locus of events, that is, of sensations, thoughts and acts, which ' take time ' and whose very nature, intimately bound up with that of the moment, is such that by merely taking place they carry us into a different moment. Thus by dynamic movement in time I mean the translation of the moment-in-being along the calendar axis; that is the outside view. Or I mean the transformation of one moment into another; that is the inside view. There is, however, another thing that we might call ' movement in time ', and this other thing is indeed my central theme. It consists in the mind's power to create images unaided by outside stimulus, and to label them with dates other than the actual date at which such images are created.

Imagination, I would say, is of three kinds. It can be, firstly, the free unlimited fantasy of day-dreams. Regarding this it is irrelevant to ask whether what is imagined could come true. Secondly, there is the imagination which constrains its images in two ways, making them in the first place compatible with the individual's beliefs about the nature of things and about human nature, so that they represent something that seems to him possible in the abstract, and in the second place attaching them to named future dates and restricting them to such transformations of his existing situation as seem to him possible in the time-span between ' now ' and ' then '. Such images are thus subjectively possible consequences of action-schemes open to the individual, and I shall call them *expectations*. Thirdly when, amongst all the courses of action open to him, the individual has chosen one and mentally committed himself to it, constrained imagination becomes *anticipation*, an experiencing beforehand of some supposed outcome of a decision.

Imagination in the two latter of these senses might seem to provide a substitute for the actuality of moments other than the individual's moment in being. Yet surely this is a delusion. Actuality is unique, expectation is multiple and uncertain. As the individual's viewpoint advances, as his moment-in-being transforms itself, the relative vividness of the rival expectational pictures which he forms of the outcome of any one action-scheme shifts repeatedly with the impact of news and the continuous process of interpreting it. How can this be a substitute for the uniquely certain actuality of a moment-in-being ? If we are still tempted to think that two actualities can exist together and be brought face to face in the same moment of thought let us suppose that the action-scheme which a man has decided on affords him by anticipation both pleasures and pains. The pleasures are associated in his mind with a calendar label $t_1$ and the pains with a later calendar label $t_3$. As he did decide on this action-scheme there was plainly some date, $t_0$, when the good anticipations outweighed the bad. But what are his feelings at a date $t_2$, part-way between $t_1$ and $t_3$? There is now nothing pleasurable, we are assuming, for him to look forward to amongst the consequences of the action-scheme but only its pains that are the price, still unpaid, of the pleasures which now belong to memory. Is it not possible that in this moment he will regret his decision ? It is not certain he will, for memory has pleasures of its own. But if, as outside observers, we see him regretting at $t_2$ the action-course that, at $t_0$, had seemed preferable to all others, we may want to ask: Which of these two views, taken at different dates by the same man about the same act, is just ? I contend that this question is meaningless. There is no co-existence of these two moments, $t_0$ which contains as part of itself the decision to adopt the action-scheme, and $t_2$ which contains as part of itself the regret for that course. These two moments cannot, for the individual to whom alone the question could matter, be ' in being ' together, the actuality of one denies and excludes the actuality of the other, and it is only by assuming ourselves to be observers standing *outside* of the system under discussion, the thought-system of this individual, and outside of the time, the moment, in which his thoughts take place, that we can make the question even seem to mean something.

If it be true that the *actualities* of two distinct dates cannot co-exist, it follows that choice between rival action-schemes must surely be based on comparison of their expected consequences. It is accordingly experiences by imagination, subjective entities, which guide decision. We cannot look upon decision as depending on a subjective evaluation of objective realities: it is subjective from the start. If the act of deciding is the act of maximising something, that something must be a feeling belonging to the same moment as the act of decision itself. The purpose, the desired effect, of decision will be experience by *anticipation*, that is, by imagination limited to the seemingly possible, and this act of imagination will be part of the moment of decision.

What is left of the distinction between rational and irrational conduct ? Conduct is judged, by an outside observer, to have been rational when it has brought consequences which, at the moment of experiencing them, the individual finds superior to any others which he now judges he could have secured. Does this mean that, in any other case, his conduct at some earlier date was ' mistaken ' ? By what right does the later moment claim jurisdiction over the earlier ? The earlier had its own enjoyments by anticipation towards which, in their actuality at the moment of decision, the later moment can contribute nothing and from which its conflicting evaluation has the right to detract nothing. The two moments are eternally exclusive of each other and wholly incomparable. Rationality means something only for the outside observer. Those who claim that, in choosing an action-scheme in face of a not-to-be-repeated set of circumstances, a decision-maker would do himself most good by applying a calculus of ' objective ' probabilities to the rival claims of various imagined outcomes of each action, are looking at time from the outside and making all the dates ' co-actual '. Would do himself most good, *when* ?

## 3 *The Nature of Decision*

Either of two suppositions would destroy the meaning with which our habits of thought and speech seem to invest the word *decision*. If the transformation of the moment-in-being as it reaches point after point of the calendar axis is no more than the rolling of a wheel along a road which constantly and fixedly exists in its entirety wherever the wheel is located, so that the human consciousness merely comes across situations, or the events which carry one situation into another, as inevitably as the traveller comes across the mountains and oceans that lie in his path, then human decisions are mere acts of recognition of the compulsion of circumstance. If we regard the history of the world and of each person in it as the unrolling of an initially complete design, as determinate and pre-destinate, then ' decisions ' can play no more vital and spontaneous role in this process than the shapes of the pieces of a jig-saw puzzle play in the fitting together of the puzzle. The final picture takes its character from the necessity of fitting the pieces together in just the one possible way; yet the profile of each piece was cut before ever the fitting together began to be attempted; the character of the ultimate picture was pre-determined, and it would be against all sense to ascribe what gradually emerges as being created by the profiles. Yet we speak and think as though decisions were *creative*. Our spontaneous and intuitive habits of mind treat a decision as introducing a new strand into the tapestry as it is woven, as injecting into the dance and play of events something *essentially* new, something not implicit in what had gone before. The two conceptions are totally opposed. Either decision is nothing or it is everything.

A supposition virtually equivalent to that of determinism is perfect knowledge. If a person felt himself to have in mind a complete specification of the entire set or range of action-schemes open to him, and for each of these action-schemes, a complete and certain knowledge of its entire consequences as far as these were of interest to himself, and if also he could order these outcomes, one for each action-scheme, according to his preferences; then his selection of one action-scheme out of all those open would, we should surely have to say, be mechanical and inevitable, and would have no content of the kind which makes our *working* ideas about decision interesting. Decision would be *empty*, the mere registering of a formal solution to a purely formal problem.

Exact, complete, and certain knowledge of what would follow from any possible act, utterly remote as this notion is from experienced life, is the tacit basis of much of economic theory. Yet when this strand is relinquished, it is exchanged only for the most worthless substitute, the assumption that, even if the individual cannot pick a unique train of consequence for each possible act, he can make a list, complete and, somehow, known to him to be complete, of all the possible trains, and attach to each of them a proper fraction giving the proportion of times, in an infinitely long lifetime of repetitive experiment, that this outcome would prove the true one. I do not want at this moment to consider the second half of this assumption, the invoking of notions of relative frequency. That is a separate matter. The true basis of the disaster wrought upon economic theory by the *games of chance* universe of ideas is the notion of the existence and the attainability of a list, complete and known to be complete, of all the possible outcomes of an action. In games of chance this possibility, of listing completely all the contingencies, is assured by the very nature of these games, their inherent and essential dependence upon a set of explicit rules. It is the completeness of this list which makes it logically possible to distribute relative frequencies over the contingencies. Once we abandon the notion of rules of an artificial game and the 'objective' list of its possible states, and mean by 'possible' outcome of an act anything that the individual thinks to be possible, we find that outcomes are not *given to* the individual but imaginatively *created by* him. And who is to predict, and how, what outcomes will be imagined by the individual for each possible one of his acts ? Who is to say when he has finished his work of creating his list of possible outcomes ? In what sense can such a list, even in the individual's own reckoning, ever be complete ?

The word *decision* surely epitomises part of our intuitive, spontaneous attitude to life, and an attitude which appears more surprising the more attentively it is compared with that other attitude which has been conditioned into us by education, our scientific attitude to life. That spontaneous and natural attitude is, I suppose, common to all of us, so our habits of speech suggest; and it is surprising only when we look at it against the background of the characteristic scientific attitude. Between man in his spontaneous naturalness, and man in his superior scientific wisdom, there is a deep divorce and alienation. Natural man believes, without thought or question, in his own creativity, in his power to conjure from the air a new strand in the texture of affairs, by merely saying *I decide*. . . . Scientific man says that he can predict the course of events. Give him an adequate description of what has been up to now, and he will tell you what will be after this. Now these two decisions are utterly at variance. One cannot suppose that the man who claims to predict allows to himself, but to no-one else, the inceptive power of genuine decision, the capacity to inject something *essentially* new into the whirlpool of events; and one cannot suppose that he believes it logically possible to foresee his own decisions while still looking upon them as decisions, as future events which will be creative in the full sense. Yet if he does not make these claims, how does he suppose himself able to predict ? Someone else's decision, or even his own, could blow his prediction to the winds.

## 4 *The World of Order and Inspiration*

If a man felt that he knew completely and for certain all the consequences, of any practical or emotional concern to him, that would flow from each given act in the range of acts open to him; and if he felt that he could rank these various trains of consequences, one for each act, according to his preference; then for him the act of choosing between these acts would be purely formal and automatic, his ' decision ' would be empty. If we opt for this assumption there is nothing in the idea of decision worthy of being discussed. All we need is an analysis of ' economic forces ', a theory of the economic system fully as independent of any specific psychic or humane elements as the theory of the solar system is.

But if not, what must we mean by decision? If it must not be choice under perfect foresight, it must be choice in face of doubt and ignorance. Yet it cannot be choice in face of chaos and anarchy; for a man who thought that any act could have any sequel whatever, and that there was no possibility of excluding *anything* as incapable of following from any stated course of action, would believe any one act to be just as eligible, just as wise and efficient, as any other, and decision would be pointless. Decision can only mean what we ordinarily do mean by it, it can only be non-empty and non-futile, in a world of bounded uncertainty. Let me then state my definition: Decision is choice in face of bounded uncertainty.

The world of pure determinism where 'decisions' are the mere clicking of the machine as it works, the world of perfect foresight where everything is actual at once and all possible experiences are in effect pre-experienced in one comprehensive explosion of events, and the world of anarchy where nothing that happens is constrained in any degree by what went before nor itself constrains what will come after, are alike fatal to that content of the word that our whole attitude to life assumes, the implicit belief that a decision is in some strict and full sense a beginning, something constrained indeed but not determined. In order to study decision in this sense, we must assume a world which satisfies two conditions. First, it must be a world where natural law operates, where a man feels that if his immediate act were different the sequel would be in some respects different, where, in fact, the course of the game can be influenced and affected, but not determined, by the individual player: in short, a world of order. But within this order, secondly, there must be room for the creative process to be still at work. For we have, it seems, the choice between two assumptions. Either creation was a single act and all that comes after was thereby settled and fixed forever; or creation is a continuing process and the shape of events can still arise from the void through men's inspired decisions. We have to suppose that into each moment in each man's mind there can be injected something *essentially* new, something not arising, in its completeness and its essence, merely as the one inevitable response to the impact of a given stimulus on the existing stock of his mind, nor as the only possible upshot of the inner working of that existing stock of thoughts, ideas, and knowledge.

In short, whether or not there is such a thing as *decision* in the full sense that our spontaneous working habits of mind give to this word, such decision if it does exist belongs only to a certain scheme of thought about the world, that which supposes in it *order and inspiration.*

## 5 *Prediction*

If, as the basis of our thinking, we opt for a world of non-empty decisions, we are denying the possibility of exact, unlimited prediction of human affairs. For a decision, we are saying, is a cut in the logical connectedness of past and future. We are saying that part of what enters into a decision is uncaused. Are decisions creative or predictable ? They cannot be both, for the meanings of the two words are mutually exclusive. Thus prediction will have to mean, for us, something other than exact calculation. But the idea that, in principle, the entire conduct of every individual, and of all individuals together, could be predicted, if we knew all there is to know about what has happened up to ' the present ', conflicts both with our experience of time from the ' inside ', as an evolving, solitary moment, and with our view of time from the ' outside ', as an extension, an axis on which a plurality, indeed an infinity, of separated points can be distinguished. For to have perfect foreknowledge of the future would surely be to pre-experience the future, to telescope all of life into a moment. It is hard to regard this even as an idea, so plainly does it conflict with the nature of consciousness itself. But perfect foresight appears even to involve logical self-contradictions. What could be meant by perfect foresight of an invention or discovery ? This is a contradiction in terms. We might say the same of the idea of perfect foresight of a decision of one's own, were it not that the concept of non-empty decision is in any case not meaningful in a deterministic world. The logical absurdity involved in supposing two people both to have perfect foresight of each other's free actions was happily shown by Oskar Morgenstern with the help of Sherlock Holmes. If the escaping criminal, Professor Moriarty, had perfectly foreseen the course of Holmes's pursuit, he could have evaded it; but if Holmes had perfectly foreseen this evasive action, he would have defeated it. The self-contradiction of the idea that several people can all use perfect foresight in pursuit of rival ends closely parallels the dilemma pointed out by Popper, that some predictions, if made public and believed, would induce action tending to falsify them. We could suppose the predictor to use his power of foreknowledge merely to choose for himself whatever course of action would most benefit him, keeping this knowledge and his own intentions secret from others. But why should we suppose that the system contained just one and only one person with this power? It could not contain several, for their respective powers of foresight would mutually cancel each other. The only possible predictor whom we could suppose to escape any danger of causing the falsification of his own predictions would be an observer entirely outside and detached from the system. And it is, indeed, only for such a person that the idea of an economic dynamics in the orthodox sense, a scheme of thought embracing a stretch of historic time and treating it as all actual at once, could have any meaning.

### 6 *Can We have an Economic Dynamics?*

Economic dynamics can be comprehensively defined only as everything that is not the analysis of a system abstracted from every meaning of time. The pure static system is one where either there are no changes, or where all changes take place instantaneously, so that all *connected* changes take place simultaneously. Nothing short of this will really do. The stationary state is a mere concession to intellectual weakness. What can an infinite repetition of moments, all identical with each other, and each perfectly self-contained in the sense that all that happens within it exhausts its effect within the self-same moment, teach us, that a single one of these moments cannot teach? You may object that in the stationary state we can suppose today's work to produce tomorrow's saleable goods, so that there is a certain temporal interlocking without change. But what is the good of such a supposition? So long as there is no change, we might just as well suppose every moment to be self-contained. The stationary state is at best as artificial as the static system, since while the latter abolishes expectation altogether, the former constrains it to such beliefs about the future as can be entertained without giving rise to change. The static system is clear-cut and goes the whole way to exclude time, the stationary state only pretends to admit it.

It would be natural now to ask whether our departure from the static system, in order to obtain an economic dynamics, should lead us to the inside or the outside view of time. I would answer that there are two conceivable classes of economic dynamics, one corresponding to each of the two views of time. It is plainly the outside view which gives us an economic dynamics in the accepted and orthodox sense. For in that sense we consider a sequence of moments to belong to one and the same actuality, and that actuality must therefore be that which is seen from the outside by the eye of an observer who is himself no part of the system; were he to include himself in his view of it, all moments except the solitary present would lose their actuality. This *exterior* dynamics falls further into three kinds. In its strict and *calculable* form the current values of all variables are made to depend on some set of their previous values, so that, if over some sufficient interval or series of dates the values of a self-contained set of variables are known, all subsequent values can be calculated. In such a dynamics there can of course be decision only in the empty sense. Inspired decision, true decision in our sense, is, precisely, an unpredictable initiative. But there is an inertia in affairs. However far a decision may depart from being an obvious reflection of obvious circumstance, its effect will take time to work through the economy, which meanwhile will swing along a path at first largely shaped by its antecedent states. It is, perhaps, only a relatively few key individuals at any time whose decisions can singly contribute to this course anything that can ever be imputed to them by our outside observer, though we must suppose, perhaps, that any man's decision can set off a chain reaction that will amount to a great effect. Nevertheless we are supposing each man to make his decisions in a world of subjectively bounded uncertainty; for each action open to him he discerns a great range of possible ultimate consequences, but a range which, within any finite horizon, is not utterly unlimited. All this may give to the sequence of states seen by our detached observer a sort of continuity of texture which will enable him without absurdity to make short-range guesses. The basis of such guesses is the second kind of exterior dynamics. Thirdly we can in a certain manner combine the first two kinds into what I would call a *ceteris paribus* dynamics. We assume like the Swedish period analysis, that at a particular instant a number of economic agents all take decisions simultaneously, and that thereafter during a certain interval everything that happens is the direct or indirect consequence of these initial decisions. By a direct consequence I mean here an act which precisely executes the initial stages of a decision, and by an indirect consequence I mean an act which is the ' automatic ' response merely to events which themselves spring directly from the initial decisions, or from the interplay of acts directly arising from those decisions, without any new decisions in the non-empty sense. In this manner we can at one and the same time bring fully into our analysis the inseverable structure of expectations and decisions and yet allow ourselves to trace consequences from antecedents on the supposition that no *essentially* new initiative interferes.

Let me turn to the *interior dynamics* of the individual's solitary moment-in-being. It can be constructed only by each person for himself, since he alone can have insight into his own mind. For this reason many will reject the notion of an interior dynamics as unscientific. Extraspection is scientific, introspection is not; sense-perception is respectable, self-perception is delusory. Why ? Descartes at any rate did not agree. He based his belief in his own reality on the fact of his own thoughts: *Cogito, ergo sum.*

The psychic solitary moment I would suppose to consist in the creation and use of expectations; to consist in imaginative creation of the set of possible action-schemes and, for each action-scheme, a bounded range of its possible outcomes; in the focusing of attention on certain *expectation-elements* of each such bounded range; and the selection of one action-scheme out of all those open, whose focus-elements of expectation will serve as the basis of anticipatory experience. I must apologise here for having had to bring in a reference to arguments and ideas which there is no space to develop in this paper.

Between an exterior and an interior dynamics there are plainly many essential differences of nature. An exterior dynamics is *public* and *objective:* the thing studied by one outside observer can be studied by another, since it exists in some sense independently of the existence of any observer. It is *mechanical,* for it looks upon each momentary state of the system as a phase in the determinate behaviour of a *machine of limited design*, a machine whose whole potentialities we can in principle know, so as to be able to tell, from information about what has happened up to now, what will happen next. And an exterior dynamics will be *aggregative*, for it deals with the totality of the actions of many individuals to each of whom the observer's own relation is the same: that of aloof and detached study. And above all, the exterior dynamics will claim to be *predictive.* In its calculable form an exterior dynamics is in a certain sense, paradoxically, timeless. The time which it considers is denatured, its vital principle of creative uncertainty, which belongs only to interior time, has been wrung out of it, so that all its infinity of distinct points along the calendar-axis are, to the outside observer, all equally actual and their ' content ' or events all equally known. Exterior dynamics of our second kind, which I would like to call inertial dynamics, is by its whole purpose predictive, though in a tentative and undogmatic way. The writer of 'inertial' dynamics invites his readers to watch for departures from the inertial course of events, he continually suggests what could happen if this and if that unpredictable impulse should strike the system from outside its defined boundaries of internal interdependence. In this use of the word *if*, he is moving away already from the role of prophet towards the task of scientific *description*. To describe the orderliness of nature: this is all that it lies within the power of the scientist, as such, to do. In this light, economic statics and economic dynamics of any and every kind are one: a means of description merely.

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# P. W. BRIDGMAN'S OPERATIONAL ANALYSIS: THE DIFFERENTIAL ASPECT *

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## *Introduction*

P.W. Bridgman's philosophy of science, the celebrated Operationalism, has become widely known in the last few decades and has been frequently the subject of discussion amongst philosophers and scientists. Some have hailed it as epoch making. Others have not seen in it much that was new and had not already been said by empiricists during the last few centuries, while yet others have considered his views not only erroneous but even harmful to the progress of science if accepted and insisted upon.

There can be no doubt that both friendly and hostile commentators have often misrepresented his views. The chief reason for this is perhaps the fact that his commentators have derived their ideas about Operationalism mostly from general expositions endeavouring to clarify its meaning in abstract terms, instead of following his applications of Operational Analysis to actual examples. Bridgman himself realises the inadequacy of his own exposition:

> It would be difficult to exhaustively characterise the operational technique of analysis; it is more of the nature of an art to be learned by the practice of it and by observation of its practice by others.[1]

Therefore it is better not to pay too much attention to what has been said about Bridgman's ideas, either by others, or even by Bridgman himself. Far more insight will be gained by following these views in their application to actual physical cases. Many of the unexpressed qualifications and restrictions that become apparent in the light of practical analysis show Operationalism to be a far less concise and clearcut system than it appears at first sight. Yet in its more modest and practical form it contains much that is new philosophically and useful in the advancement of science.

* Received 28. v. 58

[1] P. W. Bridgman, *Reflections of a Physicist*, New York, 1955, p. 25

## 1 *A Means of Distinguishing between Differences in Meaning*

Here I propose to examine an important aspect of Operationalism occurring very often in Bridgman's analysis yet hardly ever referred to explicitly either by him or by other authors. For the sake of brevity I will call it the ' differential ' aspect.

Operational Analysis is commonly understood to consist of an attempt to redefine all our scientific concepts in terms of the operations giving rise to them, rejecting concepts which are undefinable in terms of these operations, as ' meaningless '. After constructing an operational criterion, by deciding on the nature of the operations that will be admissible we are to examine our concepts in the light of it and classify them into ' meaningful ' and ' meaningless ' ones. A typical resumé along these lines is for instance that of Frank who says:

> . . . a theory which does not contain the operational definitions of its abstract terms is meaningless. In this way he (Bridgman) arrived at a concept of ' meaning ' and ' meaninglessness ' that was similar to Carnap's.[1]

But the above indication of the nature of operational analysis does not take into account the much wider scope of analysis, namely that of revealing the different operational elements amongst the ' meaningful ' concepts themselves. For a concept or statement is not just either meaningless or meaningful—according to Bridgman—there are also different sorts of meanings depending on the operations involved. By discovering these differences, we may also anticipate hitherto unsuspected practical differences between the properties associated with various concepts.

Before explaining further the nature and scope of this ' differential ' aspect with the help of some practical examples, it will be interesting to read an attack on Bridgman by Margenau which represents a most common criticism on Operationalism and which results from the complete disregard of the differential aspect.

> Precisely what is meant by an operation is of concern to those who wish to understand Bridgman's views. Originally there seemed little doubt that experimental operations, processes leading to measurements, were envisaged. Later, however, Bridgman has spoken of paper and pencil operations and even of mental operations. This seems like a retreat, for if thoughts were included among the operations, nobody could possibly find fault with operationalism *nor would it be saying much* (italics mine).[2]

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[1] P. Frank, *Modern Science and its Philosophy*, Cambridge (Mass.), 1950, p. 44

[2] H. Margenau, *The Nature of Physical Reality*, New York, 1950, p. 232

Reading Bridgman's analysis will indeed show that he does admit all sorts of operations. Besides the actual instrumental operations, he sometimes allows us to use operations not performable practically but only possible in thought—mental operations. Further, mathematically derived concepts are considered by Bridgman as operationally defined—in a paper and pencil sense. But his efforts are being directed towards detecting the different operational elements that enter into a definition and separating them from each other. For the existence of different operational elements is a most important clue for Bridgman for the existence of difference in meaning which eventually will manifest itself in fundamental practical differences.

A simple example of this ' differential ' aspect and how it can lead to practical results is provided by Bridgman when he proposes to resolve the well known ' Gibbs Paradox ' in thermodynamics. It illustrates also how misplaced was Margenau's criticism that ' nor would it be saying much ':

> Given a box with a partition in the centre and on the two sides of the partition two different gases. The partition is removed and the two gases allowed to diffuse into each other. Complete mixing is accompained by a characteristic and easily calculable increase of entropy, the same no matter what the nature of the two gases. Hence the increase of entropy must remain the same in the limit when the two gases become identical with each other. Here is the paradox, because the parts of a single uniform gas are continually inter-diffusing into each other, and there is no increase of entropy.
>
> To resolve the paradox, we must recognize in the first place that we have here a question of double limits and that the limiting process has been improperly performed. In the original set-up the universe of operations includes the operation by which the gases, originally in the two halves of the box can be distinguished from each other, i.e. the operations by which two 'different ' gases can be distinguished. It is assumed that this operation can still be performed and has meaning during all stages of the limiting process, as the two gases are allowed to become more similar. Of course this condition becomes more and more difficult to realize instrumentally, but verbally the formulation remains the same. But in the final step of the limiting process there is an abrupt discontinuity in the instrumental process: the two gases now become ' identical ', which means that there is no instrumental operation performable now by which they may be distinguished, but the instrumental operations by which two molecules are to be distinguished would demand a determination of the history of each molecule,

> according as it was originally in the right or left hand side of the box. Such operations are only pencil and paper operations, not the laboratory operations on which thermodynamics is predicated and which give entropy its meaning. Since the universe of operations of the limit differs discontinuously from that of the preceding stages, the argument for the continuity of any function such as entropy fails.[1]

It is important to follow Bridgman's argument in full detail in order to appreciate the difference between his attempt to solve the paradox and the attempts of others. For others have also based their answer on a discontinuity in the limiting process but their solutions are merely *ad hoc.*

Max Planck[2] for instance, after showing that the increase of entropy at the diffusion of two gases is given by $(-n_1 \, lnc_1 - n_2 \, lnc_2)$—where $n_1$, $n_2$ are the number of molecules $c_1$, $c_2$ are the concentrations of the two gases respectively—says:

> It also appears that the increase of entropy depends wholly on the number of the molecules $n_1$, $n_2$ and not on the nature, e.g. molecular weight of the diffusing gases. The increase of the entropy does not depend on whether the gases are chemically alike or not. By making the two gases the same there is evidently no increase of the entropy since no change of the state ensues. It follows that the chemical difference of two gases or in general two substances cannot be represented by a continuous variable. . . .

Thus he states quite clearly that the discontinuity *follows* from the evident absence of increase of entropy and not the opposite way round as it is with Bridgman. For, once we have adopted an ' operational attitude ', then without ever having been made aware of the lack of increase of entropy on self-diffusion, thus not being puzzled by any paradoxes at all, the difference in meaning, that might exist between the process of ' diffusion ' when applied to different gases and when applied to elements of the same gas, would present itself to our mind. For with a complete absence of convection currents how do we know at all that a continual intermingling of the elements of a gas takes place ? This follows from our kinetic picture of gases, according to which a gas, at complete rest macroscopically, consists of molecules rushing about at enormous speeds. But knowledge based on a mental picture is essentially different from that confirmed by instrumental evidence, whereby we detect intermixing of different gases. Admittedly we would be in no position to foretell the nature of the manifestation—if any—of such difference in the meaning of 'diffusion', but when presented with the paradox all we have to do is to correlate the abrupt change in the behaviour of our system with regard to entropy increase, with the abrupt change in the meaning of the process taking place.

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[1] P. W. Bridgman, *The Nature of Thermodynamics*, Cambridge (Mass.), 1943, pp. 168–9

[2] M. Planck, *Treatise on Thermodynamics*, Dover Publ., 3rd edn., p. 222

Planck's explanation, however, is an *a posteriori* one. Furthermore his justification of the existence of such discontinuity, namely, that chemical constitution varies in whole numbers,[1] does not satisfactorily explain the special significance of the discontinuity of the last step in the limiting process, because throughout the whole process of approaching similarity we are dealing with a variation in terms of whole numbers.

Or take a different application of the differential aspect of operational analysis of a far wider generality. Even though it is hard to imagine an actual case where the following application can be made practical use of, it is an important indication of how Bridgman's philosophising about science has a bearing on doing science:

Let us say we have established experimentally Law A and derived from it by the logical steps of mathematics Law B. Can Law B now be regarded as having been tested experimentally ?

In his discussion Bridgman uses the example of a body falling freely by gravity. Say we have established that $d^2s/dt^2 = g$ by various experiments. Now we integrate twice and obtain $s = \frac{1}{2}gt^2$. Do we have to confirm this result ? This is what he says:[2]

> I think a common answer could be that verification is quite superfluous. . . . I think a better answer would be that the answer depends on the physical operations which we have chosen to employ to our verification, first of the differential equation and then of the integrated form . . . if the set of readings made to verify the differential equation is not the same as made to verify the integrated equation, then verification is not superfluous.

Of course we may object that one can never know with certainty that a differential equation represents a physical situation, as the idea of a limit is a mathematical one. Physically we can only approximate to it. Thus $ds^2/dt^2 = g$ cannot be known with certainty. This difficulty is being noticed by Bridgman.

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[1] See also M. Planck, *Treatise on Thermodynamics*, Dover Publ., 3rd edn., p. 24 n.

[2] P. W. Bridgman, 'The Nature of Some of Our Physical Concepts', this *Journal*, 1951, **2**, p. 251

The last two examples already show that, even conceding to Margenau that by admitting ' thought experiments ' Bridgman has left us no non-operational concept at all, this differential aspect of operational analysis has thereby not diminished its significance.

In fact the opposite is true. The scope of the analysis is widened by the admission of thought experiments and fruitful results can be obtained even in mathematics by considering the different operational elements of different mathematical concepts. Bridgman considers only physical examples, but it is easy to think of examples in mathematics. Take for example the concept ' sum '. We know that only after the strange properties of infinite series had kept the seventeenth and eighteenth century mathematicians baffled for a long time, did they start to realise that the ' sum ' of an infinite series must be a different concept from the ' sum ' of a finite series. It is not really a ' sum ' in the same sense but a limiting value.

Now the application of operational analysis could reveal this at once, for the meaning of ' sum ' depends on the operation of summing. In the case of finite series summing can always be performed by adding one term to the next, their sum to the next, and so on. With infinite series, however, the process of summing is never actual addition of the individual terms one to the other but some different operation such as the elimination of all the terms except the first and last or integration and the like. Thus the difference in the operation of ' summing ' might have served as an indication of the existence of basic differences in the nature of the two concepts.

## 2 *A Criterion of Physical Reality*

This leads us to an important point in Bridgman's view on the differential power of operations, a point which has again been often ignored by followers and opponents alike. Two different operations only indicate the possibility of differences in the nature of the concepts but they do not necessarily imply such differences. If careful examination shows for example that the concept length, specified by the operation of ' moving a meter stick from one position to the other maintaining itself always parallel to itself ', and the concept length ' determined by rotating the stick through 180° for each successive position ', do not differ from each other in any way, there is no point in maintaining that we are dealing with two different concepts. Initially we are to be careful and not to take for granted that we are just dealing with two different measurements of one and the same concept but say perhaps one measurement represents ' parallel ' the other ' rotational ' length and expect to find differences between the two just as modern physics did find differences between ' tactual ' and ' optical ' length, i.e. between distances measured by meter sticks and distances measured by means of Doppler Effect. But when no such differences are revealed then not only should we not insist on the existence of two concepts but on the contrary we should consider ourselves as having proved that we are confronted with a ' real ' property, namely length is a real property as opposed to an ' invented ' property which is devised for correlational purposes. In fact the main bulk of Bridgman's writing on thermodynamics is devoted to finding out which of the thermodynamical concepts are ' real ' and which not. He examines in turn such concepts as ' energy ', ' entropy ', and the test to be applied is always the same: If we can find two independent operations in terms of which the same concept can be defined then we know that the concept is a ' real ' one. For his position seems to be that since properties are best characterised by operations—two operations should be expected to give different results, and if they do not, then by virtue of the differentiating power of operations, we have the strongest evidence that we are dealing with a ' real ' property transcending the methods defining it. Thus we are entitled to ascribe physical reality to the concept of ' stress ' of elastic bodies, because it is uniquely connected with two independent sets of operations. For one way of defining the state of stress of a body is in terms of the forces acting across its free surface. But as a body under stress is also in a state of strain which may be determined from its external deformations, we are provided with an additional means of defining stress.

It would be interesting to enquire what the ' reality ' of a concept or property actually implies, yet without further enquiry it is quite clear from the above that any attack along the lines of Lindsay's attack on Bridgman is unjustified.

Lindsay claimed[1] that the Operationalism of Bridgman implied the tying of a concept to a definite operation. Consequently the temperature of a body measured by Pt.-wire-thermometer means something different from its temperature when it is measured by a Hg-thermometer.

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[1] R. B. Lindsay, ' A Critique of Operationalism in Physics ', *Philosophy of Science*, 1937, 4

> But such an isolation of concepts could defeat the very aim of physical science, which is to provide a simple and economical description in terms of a minimum number of concepts.

He continues by saying how much effort has been expended to divorce temperature from particular operations:

> . . . culminating in the absolute thermodynamic scale of temperature which is independent of the thermometric substance used. Even more recent is the attempt to define the concept statistically. Now it is hard to believe . . . that these have not deepened our understanding of thermal phenomena. Nevertheless strict adherence to the operational point of view would place them outside the pale.

The above criticism could apply against some of the followers of Bridgman in whose opinions Operationalism does necessarily lead to such extreme attitudes. Thus Dingle, in an article 'A Theory of Measurement'[1] does insist for instance that distances measured directly by laying measuring rods end to end and distances measured by triangulation method where we observe the angles subtended at the eye by the two extremities and then apply calculations are not the same but two distinct quantities which, if we are to be consistent, ought to be called by two different names even, such as, say, 'distance' and 'remoteness'. Or for example he explains that:

> . . . the results yielded by our ten methods of finding surface tensions are not ten strivings after the same ideal but ten independent quantities which we have discovered to be approximately equal.

Not so Bridgman. To him it is only important that we should not take it for granted that the different ways of measuring temperature measure one and the same quantity, but once it has been established that the results of the different measurements differ in no appreciable way one from the other we should expect further manifestations of the very same quantity. Thus the absolute thermodynamical scale of temperature is not outside the plan of operationalism, for Bridgman does not hold that 'temperature is what the thermometer measures' just as he disagreed[2] that 'intelligence is what the intelligence tests test'.

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[1] H. Dingle, 'A Theory of Measurement', this *Journal*, 1950, **1**, 5

[2] P. W. Bridgman, 'Some General Principles of Operational Analysis', *Psychological Review*, 1945, **52**

# NOTE AND COMMENT

## *A Theory of Causality*

The view of causality that I shall describe here is a modification of the classical view. The classical view has strong overtones of strict determinism. For example, the throwing of a stone may be regarded as the cause of a window being broken, and the classical analysis would tend to ignore the fact that the window may after all fail to break. If the possibility of this failure to break were taken into account in the analysis it would be ascribed to the fact that other causes were at work. There is, however, a more modern view of causality in which determinism is not taken for granted. The modern or modified view seems to me to be a more useful one than the classical one.

If I speak later about a ' definition ' of causality this should not be taken as implying that no other definition is possible, and many readers may prefer to regard the discussion as about a ' theory ' of causality instead. But if I am not mistaken the definition corresponds closely to modern English usage. It is just because the definition is mathematical that it cannot be found in any English dictionary. Of course the analysis is not especially tied to the English language.

In a conversation in Chicago in 1955, L. J. Savage remarked that the understanding of probability had reached a point where it should be possible to give a definition of causality. We agreed that (i) the thing to try to do would be to define the expression ' $F$ is *a* cause of $E$ ' (not necessarily *the* cause), where $E$ and $F$ are events, (ii) the definition should be probabilistic, (iii) if we could prevent $F$ we would tend to prevent $E$, (iv) $F$ seems to precede $E$ in time, (v) it is inadequate to say that $E$ and $F$ are correlated and that $F$ precedes $E$ in time, a counter-example being the sensations of lightning and thunder.

I have recently given more thought to the question and have decided, in agreement with Popper, that the definition should be formulated in terms of what he calls ' propensity ',[1] i.e. what Ramsey[2] called a ' chance ', has sometimes been called ' physical probability ',[3] and may also be called ' material probability '. This last expression has the advantage of completing the parallelogram of expressions ' logical implication ', ' material implication ', and ' logical probability '.[4]

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[1] K. R. Popper, *Proc. Ninth Symp. of the Colston Research Soc.*, London, 1957

[2] F. P. Ramsey, *The Foundations of Mathematics*, London, 1931, p. 206

[3] See, for example, I. J. Good, *Probability and the Weighing of Evidence*, London, 1950, p. 116

[4] In order to prevent misunderstanding I should like to mention that I agree with Ramsey that physical probability can be defined in terms of subjective (= personal)

One popular notation for a subjective probability or credibility is $P(G/H)$, read 'the probability of $G$ given $H$', where $G$ and $H$ are propositions. When a proposition asserts the occurrence of an event we may use the same symbol to represent both the event and the proposition.

If $H_1$ represents all the true laws of nature then $P(E/F \,.\, H_1)$ can be abbreviated to $P(E/F)$ when we are talking about physcial probability. (I am here using the usual logical notation of a period for 'and', and $\bar{E}$ will represent the negation of $E$.)

Although indeterminism is fashionable people still talk usefully about causality, blame, and responsibility. Campbell[1] pointed out that causality is less important for science than was at one time thought, as for example, by Mill.[2] Russell[3] too says that the idea of causality is useful only 'in the infancy of a science'. But the attempt to define causality is still of interest as an example of mathematical philosophy (rather than of the philosophy of mathematics), and also as a contribution to the definition of the direction of time.

When we try to define the statement 'an event $F$ is a cause of the event $E$', it is desirable to avoid reference to time, since Reichenbach[4] defines the direction of time by assuming that causality is already defined. (That time has a direction means roughly speaking that there is no ordered cycle of events each of which causes the next one in the cycle.)

In the first place we must deny Hume's law that $E$ and $F$ must be contiguous in space and time. Perhaps Hume's law is true for strict causality but the present discussion is concerned primarily with probabilistic causality. For example, a murderer presses a trigger: his victim drops dead. This is cause and effect by common parlance, but the events are not contiguous.

We do not say that an event $F$ caused $E$ unless both events occurred. Accordingly our first pair of conditions is:

> C1 and C2. $E$ and $F$ both occurred or will occur, in other words the propositions $E$ and $F$ are both true.

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probability. It can also be defined in terms of multisubjective probability, and in terms of credibility (= logical probability = impersonal rational intensity of conviction). Analogously, the physical world can be defined for me, if at all, in terms of my sensations. I have the purely metaphysical belief that the physical world exists and that physical probabilities exist. (I also find it mentally healthy to believe in credibilities and to think of my subjective probabilities as judgments of credibilities.) If this view of physical probability is not accepted the following arguments will not be greatly affected.

[1] N. R. Campbell, *Physics: the Elements*, Cambridge, 1919

[2] John Stuart Mill, *A System of Logic*, Vol. 1, 1879, p. 377

[3] Bertrand Russell, *Mysticism and Logic*, 1949, p. 194

[4] H. Reichenbach, *The Direction of Time*, Berkeley and Los Angeles, 1956

As before, let $H_1$ state all the true laws of nature including the ones we do not know. Let $H_2$ specify the essential physical background or physical circumstances that are liable to be taken for granted, for example, that there was no lead sheet between the murderer and his victim. Let $H = H_1 . H_2$. We shall always regard $H$ as ' given ' in the probability notation, so that all our probabilities may be regarded as more or less physical. The next pair of conditions expresses the fact that neither $E$ nor $F$ *had to* occur.

$$C3 \text{ and } C4. \quad P(E/H) < 1, P(F/H) < 1.$$

We may suppose either that $H_2$ is an incomplete description of the physical circumstances, or we may suppose that it is complete and accept indeterminism. Conditions $C3$ and $C4$ will then not commit us either to determinism or to indeterminism. ' Pseudo-indeterminism ', a form of determinism indistinguishable in principle from indeterminism,[1] will also not be ruled out. This is just as well since the idea of causality should be available to us whatever our metaphysical views may be concerning determinism.

The next condition states in our example that the victim was more likely to die if the trigger was pressed than otherwise!

$$C5. \quad P(E/F . H) > P(E/\bar{F} . H).$$

If we had $P(E/F . H) = 1$ we would be trying to define old-fashioned (= strict) causality.

The next condition expresses the desideratum that an event is not a cause of itself.

$$C6. \quad F . H_2 \text{ does not logically imply } E, \text{ although } F . H \text{ might do so.}$$

The six conditions proposed above are by no means enough, not even if we break our resolution and say that $F$ precedes $E$ in time. For $F$ may be the sensation of lightning and $E$ that of thunder. We need a condition that states, roughly speaking, that we could have prevented $E$ by preventing $F$. So there exists a conceivable event, $G$, where

$$C7. \quad G \text{ did not occur}$$

(Rip Kirby failed to arrive in time!), such that $G$ would have prevented $F$:

$$C8. \quad P(\bar{F}/G . H) = 1, \text{ or approximately } 1,$$

and such that if there is no interference $F$ did not *have to* happen:

$$C9. \quad P(F/\bar{G} . H) < 1,$$

and finally such that the question of Rip Kirby's arrival would have been irrelevant if the murderer had not been there:

$$C10. \quad P(E/G . H) = P(E/\bar{F} . G . H) = P(E/\bar{F} . \bar{G} . H) = P(E/\bar{F} . H),$$

or approximately so.

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[1] See *Journ. Roy. Statist. Soc.*, Ser. B, **20**, 1958, 35, 38

In the thunder-and-lightning example we might try taking $G$ as the tight closing of the eyes. Then the condition $P(E/G \, . \, H) = P(E/\bar{F} \, . \, H)$ would fail.

The following more complicated example looks at first sight as if it wrecks the definition: Let $E =$ the sensation of thunder at noon on Monday; let $F =$ the sensation of lightning at noon on Monday together with thunder at noon on Tuesday; let $G =$ no sensation of lightning at noon on Tuesday. But in fact $G$ is relevant to $E$ if $F$ is false.

Thus (probabilistic) causality can apparently be defined in terms of physical probability, without reference to time. It is not a tautology that the future cannot affect the present; if it is true it is a law of nature.

Mr L. J. Savage has pointed out that conditions C7-C10 add nothing to C1-C6 since we can take $G = \bar{F}.B$, where $B$ is any event of probability strictly between 0 and 1. I then tried the modification of adding to C8 the condition that $G.H$ must not *logically* imply the falsehood of $F$ (the equations in C10 then being only approximate). But then Mr E. M. L. Beale showed that C7-C10 still appear to be vacuous. The reason is that, given C1-C6, there exists $G$ that prevents $F$ and also reduces the probability of $E$ to make $P(E|G.H) = P(E|\bar{F}.\bar{G}.H)$. This equation appears as a 'numerical coincidence', so that $G$ is artificial.

What I had in mind was that $G$ should provide a 'natural' method of preventing $F$, but it is difficult to give a formal definition of 'natural'. Perhaps we should say, not merely that $G$ exists, but the conditions must also be satisfied whenever the parameters in $G$ are varied slightly. It seems possible that there is an essential vagueness in the notion of causality. At any rate the present note may help to pinpoint the difficulties.

$E$ may be a cause of $F$ since $G$, $F$, $E$ may be in reverse time order, but the laws of nature are different when time runs backwards. Apparently causality and time-direction should be axiomatised jointly.

I. J. Good

# DISCUSSIONS

## Corroboration Versus Induction

In a recent note 'The Paradoxes of Confirmation' (this *Journal*, 1958, **35**, 227-233) H. G. Alexander discusses the problem of what evidence constitutes an empirical support to a hypothesis. His two main points are these. He criticises J. W. N. Watkins' view on the matter; and presents an alternative view. Further, he also claims that Popper's theory of testing is yet another form of induction. Before proceeding to refute each of these points, I should like to say this. Alexander seems to base his criticism of Popper's theory solely on the brief remarks of Watkins. Had he taken account of Popper's own papers, at least of the one in this *Journal* to which Watkins refers, I do not believe that he could have arrived at his conclusions, and the present unfortunately lengthy comments would have been quite redundant.

### I

It may be tempting to accept intuitively the following two propositions:

(*a*) The hypothesis 'All ravens are black', for example, is empirically backed by observing a black raven but not by observing a white shoe.

(*b*) Any hypothesis has the same empirical status however we formulate it verbally.

I shall take (*b*) for granted throughout the present paper and discuss in the present section the reasons for accepting or rejecting (*a*).

It seems (as Nicod suggested) that *an observation backs a theory if and only if it satisfies it or is an instance of it*, in the sense that a black raven is an instance of 'All ravens are black'. This suggestion of Nicod seems to explain or justify (*a*), for we tend to regard a black raven but not a white shoe as satisfying, or as an instance of, our hypothesis. Yet this is a mistake, as Hempel showed. 'All ravens are black' can be reformulated as 'All non-black things are non-ravens' and then it will be satisfied or instantiated by any object which is a non-black non-raven—for instance, a white shoe. Hempel concludes that on Nicod's suggestion *every object which does not refute a hypothesis supports it.*

This conclusion of Hempel's is known as Hempel's paradox of confirmation, or simply *the* paradox of confirmation. The word 'paradox' is used here not in the sense of 'logical paradox' but in an everyday sense, so that it is not obvious what a philosopher intends to avoid when he says he intends to avoid the paradox of confirmation. He may intend to deny Hempel's conclusion from Nicod's suggestion, or he may try to avoid the inconsistency involved in the acceptance of both (*a*) and Nicod's suggestions. It is clear from Hempel's argument that we have to reject the conjunction of (*a*) and Nicod's suggestion; we may, however, choose one of the following three alternatives. First, we may accept Nicod's suggestion while rejecting (*a*) itself. Secondly, accepting (*a*) we may reject Nicod's suggestion as an unsuccessful attempt to explain or justify it. Thirdly, we may reject both (*a*) and Nicod's suggestion.

Hempel accepts the first alternative: he accepts Nicod's suggestion and rejects (*a*). Watkins rejects the first alternative, for reasons to be discussed in the following sections. He does not state explicitly whether he accepts the second or the third alternative, namely whether he accepts (*a*) or rejects it as well.

Now Alexander shows that (*a*) is in conflict, not only with Nicod's suggestion (which Watkins rejects), but also with Popper's suggestion (which Watkins offers in its stead).

Popper suggested that *a piece of evidence backs a hypothesis only if it was obtained through an unsuccessful attempt to refute it.* Now it is quite conceivable that we shall see a white object which looks like a raven, and, rushing to examine it in order to refute ' All ravens are black ', find that it is not a white raven but, say, a white shoe (or, better, a white bird which looks not unlike a raven). We may examine a cupboard having good reasons to suspect that it contains a non-black raven and find that it contains only a white shoe. In these cases we shall, according to Popper's suggestion, accept the evidence as backing our hypothesis, quite in opposition to (*a*). Alexander concludes that by Popper's suggestion *every piece of evidence which does not refute a hypothesis* may *back it.*

So far I fully accept Alexander's argument. I now wish to discuss a further conclusion which he draws from the above, namely ' that Watkins does not succeed in solving or dissolving the paradoxes ' (p. 229). I find it somewhat surprising that having succeeded in drawing from Popper's suggestion the conclusion that any object *may* provide supporting evidence non-refuting, Alexander finds it sufficient to declare this conclusion to be an argument against Popper's suggestion and thus close the critical part of his paper with no further explanation.

It seems to me that Alexander *either* (i) claims that Watkins is inconsistent in accepting Popper's suggestion (only unsuccessful attempts to refute a hypothesis support it) while rejecting Hempel's conclusion (all non-refuting objects support a hypothesis); *or* (ii) that Watkins is inconsistent in accepting Popper's suggestion while accepting (*a*); *or* both. Since Alexander rejects Popper's suggestion with no further explanation, and since he criticises Watkins for supposedly accepting (*a*) (p. 233, top of page), he seems to be launching both criticisms at one and the same time. But in both cases he is wrong.

As to the alternative (i): Hempel's conclusion from Nicod's suggestion is: every object which does not refute a hypothesis *supports* it. Alexander's conclusion from Popper's suggestion is: every object which does not refute a hypothesis *may* support it. The two are by no means identical and we may reject the one and admit the other.

As to alternative (ii): Although Watkins did not state explicitly that he rejects (*a*), he did reject it by clear implication. In the passage which serves as Alexander's main text (p. 228), Watkins says (*Philosophy*, 1958, **35**, 351) that ' on a Popperian theory . . ., statements about non-ravens which do not report tests of our hypothesis [' All ravens are black '] cannot confirm it ' (i.e. support it). Referring to this very statement Alexander claims (p. 233) that Watkins ' says that statements about non-ravens cannot confirm it [i.e. support the hypothesis ' All ravens are black '] because they do not report tests of the hypothesis'. I find it strange that Alexander mis-states the main text on which he bases his criticism, especially in view of the following two facts. First, he labels his mis-statement as 'Watkins' position as actually stated by him' (p. 233 first line). Moreover, Watkins' statement occurs in a bracketted passage (Alexander omits the brackets) which refers to a previous paper in which Watkins gives his explanation of why (*a*) sounds plausible, just as Alexander gives (p.232) his explanation of why (*a*) sounds plausible. (Both of them refrain from an explicit rejection of (*a*).)

Watkins' explanation of why (*a*) sounds plausible (*Philosophy*, 1957, **33**, 118) is this.

Assume that in an attempt to refute 'All ravens are black' we ask a zoo-keeper whether he has ever seen or heard of a non-black raven, assuming such a person to be the most likely to know of such a bird if it exists. His answer, if it amounts to the assertion that he had never seen a non-black raven, will normally be something like this: 'No; all ravens I have ever seen or heard of are black.' This makes (*a*) sound plausible. Yet, Watkins adds, if we merely ask the man to tell us about the black ravens he has seen or heard of, his report 'I've seen hundreds of thousands of black ravens' will *not* count as backing the hypothesis. (Thus, again, by implication Watkins definitely rejects (*a*).)

My own position is even more radical than Watkins'. I would reject (*a*) even if it were perfectly acceptable on intuitive grounds, because I do not accept intuition as the high court of appeal. If I were to defend my rejection of (*a*) on intuitive grounds I should do it in roughly the following way.

Consider Galileo's hypothesis: it is the assertion that all freely falling bodies move with a constant acceleration. Had we accepted Hempel's view, we would consider the behaviour of all non-freely falling bodies—say, the bow in the hand of the violinist—to provide empirical backing to Galileo's hypothesis. This is evidently unacceptable. On the basis of (*a*) we would just repeat one experiment in order to back the hypothesis. In fact, more and more ingenious ways of testing it were invented. The most beautiful example is, perhaps, Newton's experiment in which a feather and a lead ball fall almost in unison in a vacuum tube which was quickly turned upside down. Another test, which refutes Galileo's hypothesis, is to compare the acceleration of falling bodies high above the surface of the earth and low in deep mines. This experiment was suggested already by Bacon, though it could not be performed then (for lack of precision instruments). Long before this experiment was performed it was clear from general considerations and from Newton's theory, that Galileo's hypothesis is false.

That much against that part of (*a*) which says what evidence is relevant. As to the part of (*a*) which says what evidence is not relevant, consider the following possible test.

Galileo's hypothesis can be tested by dropping steel balls off an electromagnet by switching off the electric current. These experiments can be carried out in deep mines to allow the steel balls to fall freely a long way. Now suppose that for a segment of its path a ball moved with variable acceleration. This would count as a refutation of the hypothesis, unless we deny that the fall was free. Suppose someone denies this. If he can show, say, that the mine contained a magnetic rock which altered the acceleration, we may surely consider this whole experiment as empirical support for Galileo's hypothesis. The support will be stronger if before carrying out the experiment we had tried to make sure, using all our knowledge of the earth's magnetism, that no magnetic rock exists in the mine.

We get a similar situation if we substitute Newton's hypothesis for Galileo's and optics for magnetism. Bradley's discovery that the apparent displacement of planets is due, not to their disobeying Newton's laws, but to the aberration of light, was considered as support for Newton's theory. Similarly, Laplace's discovery that the alleged irregularities of planetary motion (i.e. their alleged violations of Newton's laws) were merely the results of inaccurate calculations, was considered as support for Newton.

These test-situations are quite analogous to these described by Alexander where a suspected non-black raven turned out, on closer examination, to be a non-raven after all. This, of course, does not imply that *all* non-ravens provide support for ' All ravens are black '—only non-ravens involved in *testing this* hypothesis may be of empirical significance for it.

## 2

I now come to Alexander's two alternative suggestions (p. 232) as to what empirical evidence may count as backing for a hypothesis. (I should add at once, perhaps, that I accept both his suggestions as necessary—they follow from Popper's suggestion—but not sufficient.) The first is that the better the evidence the more improbable it must be. The second is that the evidence has to be improbable not given that the hypothesis is true, and probable given that the hypothesis is true; the greater the difference between the probability of the evidence given the hypothesis, and its probability not given the hypothesis, the more the evidence backs that hypothesis.

Alexander says (p. 232), that ' the probability of something being a raven is less than the probability of its being non-black ' *then* ' this is a black raven ' will, as the less probable evidence, support ' All ravens are black ' more than ' This is a non-black non-raven '. Thus, Alexander claims, on either of his suggestions, he escapes Hempel's conclusion. But I must confess that I fail to see his point. The evidence in question is not ' This is a non-black non-raven ', but rather ' This is a white shoe '—as highly improbable a piece of evidence as ' This is a black raven ' !

Let us repeat Alexander's example by which he repudiates Popper's suggestion. Assume that we look for improbable evidence, and that we know it to be very (though not entirely) improbable that we shall find anything at all in the cupboard. Searching it for black ravens we find a white shoe in it. Since it was very probable that we should find nothing there, it was improbable that we should find a white shoe there. Thus, by Alexander's first suggestion the evidence should count as backing the hypothesis. Similarly, by his second suggestion the evidence again should count, since the same improbable white shoe becomes more probable on the basis of any hypothesis which excludes some possibilities. Thus, by his own criticism of Popper's view his two suggestions have to go. Moreover, his two suggestions, far from explaining (*a*), actually contradict it.

Watkins rejects Hempel's view on the following grounds. However you limit the field of acceptable evidence, within this field there will exist evidence not conflicting with our hypothesis, and perhaps also evidence which does conflict with it.

Unless we demand the search for refuting evidence, we can always search, within the field of acceptable evidence, only for that evidence which does not conflict with our hypothesis, and be blind to the refuting evidence. The only way to rule out this tendency to find empirical backing to a false hypothesis which we can refute is to limit our evidence to that gained by attempts to refute it. Alexander's idea that (*a*) is often acceptable would make it possible to back superstitions of all kinds, like faith healing, by marshalling evidence in accordance with (*a*). Alexander's idea that the evidence must be improbable will not help, since evidence for faith healing is as improbable as any. Even his second suggestion will not exclude faith healing as empirically backed. Pseudo-science will thus become science on the basis of Alexander's suggestion. Moreover, they will turn scientific hypotheses into pseudo-science. For, as Galileo's theory was once backed by experiments, people would have only to repeat these experiments (with irrelevant variations) in order to diminish the likelihood of it being refuted. On Popper's view such experiments, undertaken in the hope of supporting the theory, would have no power to support it, and any attempts to back it will have to consist of sincere attempts to refute it.

To conclude, I think that Alexander's view is hit by the same criticism which he tries to launch against Watkins. Moreover, as Popper had already shown, in his 'Notes on Confirmation or Corroboration' in this *Journal* (especially, 1954, **18**, 144, 148, 149 n.), Alexander's conditions for the acceptability of supporting evidence follow from Popper's conditions. Had Alexander taken account of the central point of Popper's three papers he could not have failed to notice that his cupboard-example is equally applicable to his own and to Popper's suggestion. For, expecting a non-black raven to be in the cupboard is the same as estimating the probability of a non-black raven to be there to be rather high. He might also have noticed that not the attributes mentioned in the hypothesis but rather the hypothesis itself has to be *im*probable.

## 3

Alexander interprets (p. 229) Popper's theory of corroboration, or empirical backing, or empirical support, as higher level induction: past failures to refute a hypothesis raise the probability of similar failures in future. Unfortunately, this misinterpretation is only too common; quite a few people have the impression that Popper's intention was to justify the authority of empirically backed hypotheses by substituting observations of tests for observations of instances. They thus attribute to him the view that *science has authority*, that we are *bound* to accept some scientific hypotheses whether we like it or not. I therefore wish to repeat his arguments both against the view that empirically backed hypotheses have authority, and against the view that the aim of science is to test hypotheses with the hope of entrenching them, rather than with the hope of refuting them.

Science, or the scientific tradition allows a young rebel of no standing to deny the truth of the most authoritative and widespread view of his time, and to offer an incredible alternative to it. I have in mind the young and unknown Einstein. Some people are wise after the event and claim (especially since Whittaker's attack on Einstein) that by 1905 Newton's theory had lost its authority, or that Einstein's theory does not exclude Newton's. Both these claims are untrue. What Michelson's experiment refuted was not Newton's theory but the conjunction of it and Maxwell's theory. Kelvin claimed in 1904 that it must be possible to explain Michelson's experiment through modifying scientific ideas other than Newton's theory (preface to second edition of his *Baltimore Lectures*), and his view was the majority view (in fact the view accepted by all save Poincaré, who had a *programme* for an alternative theory but not an alternative *theory*). The history of the lively discussion between the Newtonians and the Einsteinians is not so old as to be entirely forgotten.

Quite generally, any attempt to attribute authority to existing views will be a discouragement to rebels who may have radically new ideas. The growth of science is the abandonment of temporarily 'established' views. Therefore we must encourage rebels, even before their success, rather then discourage them by insisting on the authority of the accepted views.

Those who deny that new scientific ideas conflict with old ones may still admit that the aim of science includes the elimination of errors and the discovery of new facts. Yet the only way to eliminate errors is by criticism, including experimental refutations. And the only way to strive for new discoveries is to attempt to refute accepted views, as I shall try to illustrate by an example.

People seldom ask how discoveries are made. I have found no discussion or explanation of how Gilbert discovered the inclination of the compass. A moment's thought will reveal that the crude instruments of his time could not possibly allow such a delicate variation to be observed without any special preparation. How did Gilbert come to make the necessary preparation? I mention this very example here because I have discussed the revolutionary philosophy of Gilbert somewhere else, alluding to this experiment (this *Journal*, 1958, **35**, 238). It may be useful to indicate how the discovery was made as a refutation of an (accepted) hypothesis. The hypothesis (of Paracelsus and Carden) was that the compass is attracted by the Pole Star. As Gilbert tells us he started his magnetic investigations in an attempt to test it. His reason, I conjecture, was that if Copernicus' hypothesis is true, then the pointing at the Pole Star of a freely suspended compass should reflect the annual rotation of the earth. (This is, of course, a rather naive estimate of the distance of the fixed stars from the solar system.) The inclination was found, but it was not towards the Pole Star. Explaining it as pointing towards the north pole Gilbert conjectured that the earth is a huge homogeneous magnet. Tests of this hypothesis of Gilbert showed the existence of unexpected variations of the earth's magnetism; thus the field of research on the earth's magnetism was opened by the refutation of Gibert's hypothesis.

This example is an attempt to illustrate how the discovery of new facts takes the form of refutations of older hypotheses (in accordance with Popper's theory). I think that my interest in correcting our errors (i.e. trying to refute them), in finding new ideas (which clash with old ones), or in finding new facts (which *are* refutations), must clash with the vulgar view that empirically backed hypotheses have any authority whatever. Science does not oppose freedom of thought but rather depends on it.

One question remains. If we are interested in empirical refutations, why have a theory of empirical support? In other words, what is the function of Popper's theory of corroboration?

There are a number of answers to this. The first is this. Popper argued that the empirical backing of which people speak is not the evidence which increases the probability of a hypothesis but that which was obtained through sincere attempts to refute it. The second is this. The more corroborated a hypothesis is the more important is the discovery which refutes it (Michelson's experiment, Hertz's photo-electric effect, etc.). The third is this. Any well corroborated theory (say Maxwell's) must be a first approximation to the theory which supersedes it: the new theory has to explain how the old theory, though false, stood up so well to severe tests (like Hertz's).

To conclude: Popper's theory of corroboration is not hit by the paradox of confirmation, but dissolves it by advancing a view of scientific thinking and research as unauthoritarian, imaginative, questioning, and rebellious; a view according to which neither black ravens nor non-black ravens nor even Michelson's experiment *confirm*, or sanctify, or give authority to a hypothesis. Popper's philosophy is a philosophy of criticism. Corroboration of a theory is merely an appraisal of the way it stood up *so far* to severe criticism. This appraisal may help those who try to criticise a theory again, for they may wish to know what happened to previous attempts. But even a high degree of corroboration should not discourage the critic. It may even be a challenge.

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## Lattice Structure of Space-time

On pages 106–107 of Abramenko's article, on the dimensionality and continuity of space and time,[1] he produces an argument that is intended to support the theory that space-time has a lattice structure. Essentially this is the ‘ chronon ’ theory mentioned, for example, by Whittaker,[2] who says:

> In fact, some physicists in 1928 made the suggestion (which, however, was not favourably received) that this might actually be the case, and they proposed the name *chronon* for a time-unit of about 4·5 x 10$^{-24}$ sec., which would correspond so to speak, to a single picture in the cinema of existence.

Clearly the chronon is $h/(m_p c^2)$, where $m_p$ is the proton mass, and the corresponding smallest space unit is $h/(m_p c) = \lambda_{cp}$, the Compton wave-length of the proton.

For convenience I quote Abramenko's argument:

> Let us consider a physical point-instant, or quantum of space-time, as an irreducible spatio-temporal volume defined by the expression:
>
> $$V_{ir} = \tfrac{4}{3}\pi l_0{}^3 \tau_0.$$
>
> Assuming $l_0$ to be of the order of magnitude of a proton radius, i.e. $\sim 1{\cdot}5 \times 10^{-13}$ cm., and the value of $\tau_0 = l_0/c = 0{\cdot}5 \times 10^{-23}$ sec., we obtain the value of $V_{tr} = 7{\cdot}1 \times 10^{-62}$ cm.$^3$ sec. If we imagine this spatio-temporal quantum to be filled with matter of nuclear density, $\sigma \cong 10^{14}$ g/cm.$^3$, its energy content will be
>
> $$V_{ir}\sigma c^2 \cong 6{\cdot}4 \times 10^{-27} \text{ erg-sec.,}$$
>
> which is of the same order of magnitude as Plank's quantum . . .

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[1] B. Abramenko, ‘ On Dimensionality and Continuity of Physical Space and Time ’, this *Journal*, 1958, **9**, 89–109

[2] E. T. Whittaker, *From Euclid to Eddington*, Cambridge, 1949, p. 41

By finishing up with an apparent numerical coincidence this argument is made to appear much stronger than it really is. The Compton wave-length of the proton may be taken as one definition of the radius of the proton. Then the nuclear density is $\sigma = m_p/(\frac{4}{3}\pi\lambda_{cp}^3)$. It is now a question of algebra (not arithmetic) to see that $V_{ir}\sigma c^2 = h$. Thus the apparent numerical coincidence is a tautological deduction from simple known atomic theory. It would have been better to present the argument thus:

> The energy in a proton ($m_p c^2$) multiplied by the 'chronon' ($h/(m_p c^2)$ sec.) is equal to $h$.

Put this way the argument may be worth something but not very much. Personally I quite like the lattice theory, but this is rather beside the point.

On pages 107-108, Abramenko leaves an argument less complete than necessary. If the total number of spatio-temporal quanta is $10^{162}$, and *if we assume that in each of them an elementary particle is created whose mass has probability $\frac{1}{2}$ of being positive and probability $\frac{1}{2}$ of being negative*, then after these have paired off with destruction, the number of particles left will be of the order of $10^{81}$. It is not necessary to assume that time is curved on itself, only that the process has been going on for a few thousand million years. For example, if space is infinite and the universe is expanding then the argument may apply to the observable universe. The question arises in this case how large each positive and negative concentration of matter may be expected to be. As a statistical problem it is rather like the problem of the distribution of sizes of connected sets (' armies ') in the game of Wei Chi (or Go) the Japanese national pastime), in which black and white pieces are placed on a two-dimensional lattice.

The weakness of the theory is that it says nothing about how far apart in space and time mutually annihilating pairs must be. Given a more complete formulation it may be possible to obtain corroboration or disproof. The theory should tell us, for example, something about the best possible vacuums. A further question is what happens if a particle tries to come into the world at a position that is already occupied ?

Regarding the question of the dimensionality of space, I find that I can place six rods of equal length to form a regular body (a tetrahedron), but I cannot add another four rods (of the same length) as I should be able to do in four spatial dimensions. This seems to show that space is locally three-dimensional, even if its structure is microscopically granular or macroscopically curved or even multiply connected.

In an earlier paper on the dimensionality of space, Whitrow [1] makes the interesting remark that the orbit of the earth would be unstable if the number, $n$, of dimensions of space, exceeded 3, and that therefore $n = 3$ is a necessary condition for the existence of the formulators of the question of why $n = 3$. Likewise Courant [2] has stated that only in three dimensions is high-fidelity signal transmission possible. Hence Whitrow's remark may be generalised to any communication systems, not just to human nervous systems.

[1] G. T. Whitrow, ' Why Physical Space has Three Dimensions ', this *Journal*, 1955, **6**, 13

[2] R. Courant in *Modern Mathematics for the Engineer*, ed. by E. F. Beckenbach, New York, 1956, p. 101

The following suggestions all seem to be worth some consideration.

(i) God selected $n = 3$ in order to make our existence, and that of other communication systems, possible; or

(ii) $n = 3$ just happens to be the case, with no explanation possible. It is a small integer and not very surprising *a priori*. If the number of dimensions had been say 386491 then we should have considered an explanation to be far more pressing; or

(iii) *we happen to be the right size* to think that $n = 3$. We do not have enough evidence to be sure how many dimensions space would seem to have if our lengths were of the order of magnitude of $10^{-24}$ cm., or $10^{24}$ cm., or $10^{48}$ cm. Perhaps for any given $n$ there could exist beings of a size for which $n$ would seem to be the most natural dimensionality, for events on a scale comparable with their own size. An analogy could be drawn with people living in greatly separated houses, numbered from 1 to 100. Someone living in a house number 17, and unaware of the existence of the other houses, might ask 'Why should a house have the number 17 on it?' (Perhaps I should mention in passing that even fractional dimensions have been suggested by Ulam [1] to have physical significance); or

(iv) even more speculatively, there may be more than three dimensions of space, perhaps even an infinite number, but we can fully appreciate only three dimensions because we ignore low-fidelity signals. I do not see how to formulate this last suggestion even reasonably rigorously, but it seems worth listing here in view of Courant's statement mentioned above.

I. J. GOOD

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## 'THE TWO THESES OF METHODOLOGICAL INDIVIDUALISM'

**HAVING** replied at some length [2] to an earlier criticism by Mr Leon J. Goldstein [3] of methodological individualism in social science as I originally put it forward in this *Journal*,[4] I shall only make one small historical comment, in an endeavour to keep the record straight, on his latest attempt [5] to cast doubt on this excellent principle.

He announces, with the air of making a discovery, that 'the fact of the matter is, however, that under the rubric "methodological individualism" Watkins subsumed two positions'; the first of these positions, Goldstein says, is not methodological at all but 'ontological', while the other is 'more truly methodological' (pp. 2-3). Later he adds that methodological individualists will never be able to come to grips with their opponents' case so long as they 'do not trouble to distinguish between the two kinds of individualism they support' (pp. 3-4).

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[1] S. Ulam in *Applied Probability*, ed. by L. A. MacColl, New York and London, 1957

[2] J. W. N. Watkins, 'The Alleged Inadequacy of Methodological Individualism', *The Journal of Philosophy*, 24th April, 1958, **55**, 9

[3] L. J. Goldstein, 'The Inadequacy of the Principle of Methodological Individualism', *The Journal of Philosophy*, 6th December, 1956, **53**, 25

[4] J. W. N. Watkins, 'Ideal Types and Historical Explanation', this *Journal*, 1952, **3**, 22

[5] L. J. Goldstein, 'The Two Theses of Methodological Individualism', this *Journal*, 1958, **9**, 33

I made this obvious distinction long ago in the paper which Goldstein originally attacked.[1] Briefly, I subscribe to a metaphysical statement and a methodological prescription, as follows:

(1) Human beings (together with their material resources and environment) are the only causal factors in history.

(2) Explain all social events in terms of human factors.

I gather that Mr Goldstein also subscribes to (1) but wishes to replace (2) by:

(2′) Explain some social events in terms of non-human factors.

The burden of his latest paper is that one can subscribe to (2′) without abandoning (1). But if one accepts that 'Ought' implies 'Can' acceptance of (2′) will involve abandoning (1), since (2′) prescribes what (1) declares to be impossible.

J. W. N. WATKINS

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[1] 'The principle whose status I have been trying to elucidate is a methodological rule which presupposes the factual assertion that human social systems are not organisms in the above sense.' 'The ontological basis of methodological individualism is the assumption that society is not some unimagined sort of organism, but really consists only of people . . .' ('Ideal Types and Historical Explanation', *Readings in the Philosophy of Science*, ed. Feigl and Brodbeck, pp. 731, 732.)

# REVIEWS

*Undecidable Theories.* By Alfred Tarski in collaboration with Andrzej Mostowski and Raphael M. Robinson.
North-Holland Publishing Co., Amsterdam, 1953. Pp. xxi + 98. 18s.

This monograph, divided into three papers I-III, originated from several combined investigations first by Tarski and Mostowski, later by Tarski and R. M. Robinson, with contributions also by Julia Robinson. Much of the results has been presented already at the meeting of the Association for Symbolic Logic in Columbus, Ohio (December, 1948). Abstracts of these reports were published in the *Journal of Symbolic Logic*, **14** (1949).

The monograph gives not only a detailed explanation of the subject together with the proofs involved, but also a certain widening of the concepts due to a slight modification of the general schema of metatheoretical investigation. Usually formal calculus and its interpretation, and correspondingly syntax and semantics are distinguished by a twofold rôle attributed to the logical laws as rules or initial formulae for the calculus, and also as rules of truth to which the definition of truth has to be adapted.

Now the two concepts of formal derivability and of truth are regarded by Tarski as special cases of a concept of ' validity ' which he takes as constitutive in the characterisation of a 'theory'. The theories which are treated in the monograph are supposed to be in *standard formalisation*, i.e. formalised in the frame of the first order logical calculus, with equality and its axioms included. Such a theory is understood to be determined by its (finitely or denumerably many) constants and by the set of its valid sentences (*i.e.*, valid closed formulas). The constants are (*a*) the logical constants, among which also the quality sign counts ; (*b*) non-logical constants, which are individual-symbols or predicators or functors.

The set of the valid sentences is assumed to include all logical identities (as also those of equality) and to be closed with respect to logical derivation.

In the case where the valid sentences of a theory can be characterised as those which are derivable from a recursive set of ' axioms ', the theory is said to be ' axiomatisable ' and in particular ' finitely axiomatisable ' if the number of axioms is finite.

But the valid sentences can also be determined by an interpretation of the non-logical constants with reference to a domain of individuals ; such an interpretation combined with the usual interpretation of the logical symbols gives every sentence formed with the constants a truth value ; and as valid sentences are defined those with the value ' true '. More generally validity can be defined by means of a *class* of interpretations of the non-logical symbols in the way that a sentence is defined as valid if it holds true for every interpretation belonging to that class. For any such definition of validity the set of valid sentences of a theory is closed with respect to semantical inference, so that the semantical consequences of valid sentences are again valid.

It is to be noticed that we need not distinguish here between semantical consequence and derivability, since we have to deal always with standard formalization to which Gödel's completeness theorem applies.

The theorems proved in the monograph are about undecidability of theories. A theory is called decidable if there is for its sentences a procedure of deciding on their validity, or what amounts to the same, if with respect to a proper Gödel-numbering of the set of valid sentences : the set of their Gödel numbers is (general) recursive.

As trivially decidable theories we have the inconsistent theories wherein some sentence, together with its negation, is valid and hence by the closure property all sentences are valid. A consistent theory is called complete if for each of its sentences A either A or $\sim$ A is valid. If a complete theory is axiomatisable, it is also decidable, as follows by an argument of Kleene. Generally a decidable theory is also axiomatisable.

Among the undecidable theories Tarski distinguishes as 'essentially undecidable' those which are not *sub-theories* of decidable consistent theories with the same constants. Here a theory $T_1$ is called a sub-theory of a theory $T_2$ if every valid sentence in $T_1$ is also valid in $T_2$, so that the set of valid sentences of $T_2$ is an extension of the corresponding set of $T_1$. Tarski therefore briefly calls $T_2$ an extension of $T_1$, when $T_1$ is a sub-theory of $T_2$. We have, however, to be aware that an extension in this sense does, in general, not amount to a widening of a theory, but often rather to a restriction. This especially appears by the various instances which occur of theories which are undecidable though not essentially undecidable. A pregnant instance of this kind, to which Tarski points, is that of a theory with only one extralogical symbol : a binary predicator P, whose only valid sentences are the logical indentities. This theory, by Church's theorem of the unsolvability of the Entscheidungsproblem for the first order logic together with Kalmár's reduction of the Entscheidungsproblem to formulas with only one binary predicator, proves to be undecidable. But in many ways we can get from it a decidable theory simply by adding a non-logical axiom, for instance $(x)(y)(x = y)$ or $(x)(y)P(x, y)$ or $(x)(y)(P(x, y) \longleftrightarrow x = y)$.

Concerning the ways of proving undecidability Tarski distinguishes a direct and an indirect method.

The application of the direct method is concentrated in one theorem (II, Th. I) with corollary, which, by the union of the syntactic and the semantic aspect, generalises a theorem of Rosser on undecidability of formal systems and at the same time Tarski's former statement of the indefinability of truth in derivative systems. The said theorem states that if $T$ is a consistent theory with terms $\Delta_0, \Delta_1, \Delta_2, \ldots$ containing no variables and for the formulas of $T$ a Gödel-numbering is fixed and a variable $u$ is distinguished, if further $D(k)$ is the arithmetic function which assigns to the number $k$ the Gödel-number of the formula to be obtained from the formula with the Gödel-number $k$ by substituting $\Delta_k$ for the variable $u$, and if $D(k)$ is definable in $T$, then the set of Gödel-numbers of valid sentences of $T$ is not definable in $T$. Definability of a set $s$ means here that there is a formula $C(a)$ in $T$, with the only free variable $a$, such that for every natural number $k$ the sentence $C(\Delta_k)$ or its negation is valid in $T$ according as k belongs or not belongs to s. In a corresponding sense 'definability of a relation in $T$' is understood. And definability of an arithmetic function $\phi(k_1, \ldots k_r)$ in $T$ means that there is a formula $\Phi(a_1, \ldots a_r, b)$ in $T$, with only the indicated free variables, such that for any natural numbers $k_1, \ldots, k_r$ the sentence $(x)(\Phi(\Delta_{k_1}, \ldots \Delta_{k_r}, x) \longleftrightarrow x = \Delta_{\phi(k_1, \ldots, k_r)})$ is valid in $T$. (By admitting descriptions we could express the last condition on $\Phi$ by requiring the equation $\Delta_{\phi(k_1, \ldots, k_r)} = \iota_x \Phi(\Delta_{k_1}, \ldots \Delta_{k_r}, x)$ to be valid for any natural numbers $k_1, \ldots, k_r$.)

The proof of the theorem is by means of Gödel's method of sharpening the antimony of the liar.

Now this theorem is applied to the formalised arithmetic of natural numbers $N$ and some subtheories of it. Here the non-logical constants are the individual symbol $O$, the unary functor **S** and the binary functors $+, \times$. The valid formulas of $N$ are those which hold with respect to the domain of natural numbers for the interpretation of $O$ as null, of **S** as the successor and of $+$ and $\times$ as sum and product.

The theory $N$, determined in this way is complete. As Gödel has shown, every primitive recursive function is definable in $N$, and therefore also every class $\{x | P(x)\}$ where $P(x)$ is expressible by a recursive relation and quantifiers, in particular every recursively enumerable class.

Since we have also in $N$ the sequence of terms $O$, **S**$O$, **SS**$O$, . . . which figure as numerals and the function $D(k)$ is (by the general properties of Gödel-numbering) primitive recursive and thus definable in $N$, all premises of theorem II.1 are satisfied for $N$. Hence by this theorem the set of valid sentences (*i.e.* of their Gödel-numbers) cannot be definable in $N$, therefore it is not recursive and even not recursively enumerable, so that $N$ is neither decidable nor axiomatisable. This result can as well be drawn from Gödel's general theorem on formal undecidability by which an axiomatisable theory wherein every primitive recursive function is definable cannot be complete. But theorem II.1 can also be applied to sub-theories of $N$ which are axiomatisable. For this application it is sufficient to show that in the considered sub-theory every primitive recursive function (and thus in particular $D(k)$) is definable. From this it then follows that the sub-theory in question is undecidable, and even essentially undecidable.

A theory of this kind is Peano's arithmetic, whose non-logical axioms are: $(x)(y)(Sx = Sy \rightarrow x = y)$, $(y)(O \neq Sy)$, the recursive equations for sum and product, but with general quantification applied to the variables, and the infinitely many sentences which are instances of the formula schema of induction.

In fact this theory $P$ (which is equivalent with the system $Z$ in *Grundl. d. Math.*) is known to have the property that every primitive recursive function is definable in it, and so it follows that this axiomatisable theory is essentially undecidable. In the present monograph the same is shown for that finitely axiomatisable sub-theory of $P$, called $Q$, which results from $P$ by cancelling from the list of axioms the instances of the schema of induction and adding instead only the sentence $(x)(x \neq 0 \rightarrow (Ey)(x = Sy))$. This system $Q$ was substituted by R. M. Robinson as a simplification to another system chosen for the same purpose by Mostowski and Tarski.

For the proof that in $Q$ every recursive function is definable a further theory $R$ is introduced which is a sub-theory of $Q$, but with infinitely many non-logical axioms: Even in this very weak theory, as is shown, every recursive function is definable and so also this theory $R$ is found to be essentially undecidable.

Further to theory $Q$, the proof is given that *none* of the sub-theories arising from it by cancelling one of the non-logical axioms is still essentially undecidable.

The essential undecidability of $Q$ results by the direct method. However this theory for itself has only a minor interest, as it is only a fragment of number theory. The consideration of it serves mainly for the passage to other theories and thus for the use of the indirect method of proving undecidability. This method consists in the application of certain theorems allowing inference from the properties of one theory to corresponding properties of another theory.

A series of theorems of this kind is proved in the first part of the monograph. In particular they concern the consequences of the interpretability of one theory in another. A theory $T_1$ is said to be interpretable in $T_2$ if every valid sentence of $T_1$ is derivable in a theory $T_2^{\star}$ arising from $T_2$ by adding to it the non-logical constants of $T_1$ which are not in $T_2$ and also axioms which are definitions of those constants, *i.e.* explicit definitions in the case of the predicators to be defined, and in the case of functors or individual symbols to be defined, such formulae as one would obtain from explicit definitions formed with the help of descriptions (introduced by the $\iota$-rule) by the subsequent elimination of the $\iota$-symbols.

A theory $T_1$ is called 'weakly interpretable' in $T_2$, if $T_2$ is a sub-theory of a consistent theory $T_2{}^{\star}$ with the same constants as $T_2$, such that $T_1$ is interpretable in $T_2{}^{\star}$.

Sometimes the interpretation of a theory $T$ in another one requires a preparation, namely when the individuals of $T$ have to be identified with only those individuals of the second theory which satisfy a certain condition. In order that this can be done by an explicit definition, we have first to make the domain of individuals of the one theory formally explicit.

This procedure consists in the following : a unary predicator P is added to the constants of $T$ ; with every sentence $\Phi$ of $T$ a sentence $\Phi^{(P)}$is correlated wherein each constituent $(x)\Psi(x)$ of $\Phi$ is replaced by $(x)(P(x) \rightarrow \Psi(x))$ and each constituent $(Ex)\Psi(x)$ by $(Ex)(P(x)\ \&\ \Psi(x))$ ; and then those sentences are defined as valid which are derivable from the sentences $\Phi^{(P)}$ correlated with sentences $\Phi$ which are valid in $T$. The theory thus resulting is called $T^{(P)}$ (' $T$ relativized to P '). In the case where $T$ is axiomatisable there is a standard process of passing from an axiom system of $T$ to an axiom system of $T^{(P)}$ ; if the axiom system of $T$ is finite and there are only finitely many functors and individual symbols in $T$, then the corresponding axiom system of $T^{(P)}$ is also finite.

Still another near relatedness of two theories occurs : that between a theory $T$ and an ' inessential extension ' of it, obtained by adding to $T$ finitely or denumerably many individual constants, but without further axioms. A theory and an inessential extension of it behave alike with respect to undecidability as well as to essential undecidability.

Now the statements on which the indirect method relies are the following

(1) If a theory $T_1$ is interpretable in a consistent theory $T_2$ or in an inessential extension of it then, if $T_1$ is essentially undecidable, the same holds for $T_2$ ; and if $T_1$ has a finitely axiomatisable and essentially undecidable sub-theory, the same holds for $T_2$.

(2) If $T_1$ is an essentially undecidable and finitely axiomatisable theory which is weakly interpretable in $T_2$ or in an inessential extension of $T_2$, then $T_2$ and every sub-theory of $T_2$ with the same constants is undecidable, and by adding finitely many suitably chosen axioms to $T_2$ an essentially undecidable theory is effectively obtainable.

(3) A theory $T^{(P)}$ obtained from a theory $T$ by relativizing to a unary predicator P (not in $T$) behaves like $T$ with respect to essential undecidability (though not generally with respect to mere undecidability).

From these theorems applications are made, in three ways, to proofs of undecidability. All these proofs are based on the essential undecidability of the finitely axiomatisable theory Q. Like $Q$ also, by (3), the theory $Q^{(N)}$ ($Q$ relativised to $N$) is finitely axiomatisable and essentially undecidable.

Let $J$ be the theory whose non-logical constants are the functors $+$ and $\times$ and whose valid sentences are those which hold with respect to the domain of integers by the usual interpretation of the functors. Then $J$ is consistent and complete. Further $Q^{(N)}$ is interpretable in $J$. In fact O, Sy and N can be defined in $J$; in particular N(x)—(x being a natural number)—can be defined as the condition that x is a sum of four squares. Hence, by (2), $J$ and each of its sub-theories with the same constants is undecidable. Among the sub-theories of $J$ which thus are found to be undecidable are the following finitely axiomatisable theories : the elementary theory of rings, and that of integral domains, both with or without the axiom of commutativity of the product, and with or without existence of a unit being postulated.

In an analogous way the undecidability of the elementary theory of ordered rings or integral domains (with the corresponding alternative conditions) can be proved. For this it is sufficient to consider, instead of $J$, the theory $J^{<}$, to be got from $J$ by adding the constant $<$ with the usual interpretation.

It is to be observed that all the said axiomatisable theories of rings are only undecidable but not essentially undecidable. In fact they all are sub-theories of the elementary theory of real numbers (in which the schema of the Dedekind continuity axiom is applyable only to predicates in standard form) ; and this theory was shown by Tarski to be decidable.

Essential undecidability is proved of the theory of non-densely ordered rings, which differs from the theory of ordered rings by the addition of one axiom excluding density. The proof goes by directly interpretating $Q^{(N)}$ in this theory and then applying theorem 1.

Finally, in Part III, the theorems 1-3 are applied for proving the undecidability of elementary group theory, *i.e.* the theory $G$ whose only non-logical constant is the binary functor $\circ$, and whose valid sentences are those derivable from the axioms $(x)(y)(z)(x \circ (y \circ z) = (x \circ y) \circ z)$, $(x)(y)(Ez)(x = y \circ z)$, $(x)(y)(Ez)(x = z \circ y)$.
The proof goes by the following steps. First it is shown that $J$ is interpretable in the theory $J^{+}$ whose non-logical constants are the individual symbol 1, the functor + and the binary predicator x/y, and whose valid sentences are those which hold by the interpretation of x/y as (x) is divisor of (y), and the usual interpretation of 1 and +. Of course $J^{+}$ is consistent and complete.

Since $Q^{(N)}$, as already stated, is interpretable in $J$, it follows that it is also interpretable in $J^{+}$ ; hence, by 1, there exists a finitely axiomatisable and essentially undecidable sub-theory of $J^{+}$ (which can be effectively constructed). From $J^{+}$ by relativising the theory $J^{+(C)}$ is obtained. Now $J^{+(C)}$ is proved to be weakly interpretable in the theory $G'$, which is obtained from $G$ by merely adding an individual symbol c and thus is an inessential extension of $G$.

For this purpose a theory $G''$ is considered which has the same constants as $G'$ and whose valid formulae are those which hold with respect to the domain of permutations of the integers upon the following interpretation of the constants : c is the permutation mapping each integer k into k + 1, and $\circ$ is the composition of permutations. Obviously $G''$ is consistent and the axioms of $G'$ are valid in $G''$, so that $G'$ is a sub-theory of $G''$. If now the constants of $J^{+(C)}$ are added to $G''$ with the defining axioms

$(x)(C(x) \longleftrightarrow x \mathbf{o}\, c = c\, \mathbf{o}\, x)$, $(x)(x = 1 \longleftrightarrow x = c)$,
$(x)(y)(z)(x = y + z \longleftrightarrow x = y\, \mathbf{o}\, z)$,
$(x\,(y)(x/y \longleftrightarrow x\, \mathbf{o}\, c = c\, \mathbf{o}\, x \,\&\, (z)\,(x\, \mathbf{o}\, z = z\, \mathbf{o}\, x \rightarrow y\, \mathbf{o}\, z = z\, \mathbf{o}\, y))$,

then in the resulting theory, which like $G''$ is consistent, every valid sentence of $J^{+(C)}$ is again valid. Namely every valid sentence of $J^{+(C)}$ is derivable from sentences $\Phi_i^{(C)}(i = 1, \ldots, r)$ correlated to valid sentences $\Phi_i$ of $J^+$. But for each of these sentences $\Phi_i^{(C)}$ by means of the axioms a biimplication $\Phi_i^{(C)} \longleftrightarrow \Psi_i$ is derivable, where $\Psi_i$ is a sentence of $G''$ which—as a nearer discussion shows—is valid in $G''$ if the formula $\Psi i^{(C)}$ holds with respect to the domain of permutations of the integers upon the following interpretations of the constants : $C(x)$ is the property of being a power of the permutation c mapping k into $k + 1$, 1 is *c*, + is the same as **o**, and $x/y$ holds if there are integers m,n such that $x = c^m$, $y = c^n$ and $m/n$. From this, however, it follows that $\Psi_i$ holds in $G''$ if $\Phi_i$ holds in $J^+$, which indeed has been assumed.

So it follows that $J^{+(C)}$ is interpretable in $G''$ and hence weakly interpretable in $G'$. Therefore by theorem (2), $G$ as well as every sub-theory of $G$ with the same constants is undecidable ; further we can add finitely many axioms to $G$, without a new constant, in such a way that the resulting theory is essentially undecidable.

$G$ itself is not essentially undecidable. In fact Wanda Szmielew proved that the theory we get from $G$ by adding the commutative law to the axioms *is* decidable.

As Tarski expressly remarks, the undecidability of $G$ does not imply the unsolvability of the Dehn word problem. This problem is not a question of undecidability of a theory in the sense here considered, but can be reduced to that about undecidability of the subset of those valid sentences of $G$ in which all the quantifiers are general quantifiers extending over the whole sentence. A solution of this problem with the result of undecidability has in the meantime been published by P. S. Novikov. Some further applications of the methods described, concerning elementary group theory are mentioned at the end.

On the whole the methods explained in the monograph prove to be very powerful. By handling the theories here described a new fruitful branch of axiomatical investigation is brought about, which yields many unexpected insights in the structure of mathematics. The presentation of this new world of concepts is very instructive and stimulating. It was a lucky idea of the editors of ' Studies in Logic ' to suggest to the authors the publication of the subject as a monograph. The book is dedicated to Heinrich Scholz.

PAUL BERNAYS

*L'Interprétation physique de la mécanique ondulatoire et des théories quantiques.*
By P. Février.
Gautier-Villars, Paris, 1956. Pp. vii + 216. 3200 fr.

TODAY the interpretation of the formalism of quantum mechanics is again being criticised. One reason for it is that established theory cannot deal with high energy phenomena and with unstable particles like mesons. Another reason is that this failure has aroused the metaphysical desires of some physicists who now propose to turn back the clock and to re-introduce determinism. Mme Février discusses here the types of interpretation that may be imposed on quantum mechanics. The outcome of her argument is, I think, convincing. No interpretation that can possibly be imagined can ever bring back classical causality. Quantum mechanics is, and remains, a statistical theory.

This is of course not a new conclusion, and the vast majority of physicists would concur in it. Moreover, Mme Février's approach is close to that of von Neumann and Birkhoff. Her arguments, however, are more detailed; and she makes use of ideas of interest to the methodologist (which were first proposed by M. Destouches and the author, see this *Journal*, 1954, 256–261).

Scientific method is concerned with constructing theories rather than with inventing isolated hypotheses. A theory must possess a domain of adequacy, that is, there exists a set of experiments the theory is capable of describing. Although no theory is completely adequate or perfect, we must make sure that the results of future measurements which are predicted fall into the domain. Only then are the results revelant to the theory and, therefore, capable of testing it. Physics develops by constructing theories which have an increasing domain of adequacy. The domain of a new theory encloses that of the old theory, and this entails that part of the formal structure is taken over from the old to the new theory. This condition expresses Bohr's Correspondence Principle in a more general form (see E. H. Hutten, *Proc. Aristotelian Soc.*, 1949, **49**, pp. 115 ff.).

Another requirement that a theory has to satisfy is that it must be capable of predicting the results of future measurements when certain initial data are given. More often than not the prediction is uncertain, that is, involves probabilities. A general method of prediction is this: the original data, e.g. of position and momentum of a particle, are first transcribed into a formal language by making them correspond to 'initial elements' of an abstract ensemble, i.e. of the wave function. Then the initial elements are changed into other abstract elements for the time for which the prediction is to hold, i.e. the wave function is evaluated for a later time. Finally the predicted values are found by a time-independent law that expresses a generalised process of spectral decomposition, i.e. a Fourier analysis.

This 'theory of prediction' is obviously based on the method by which the Schrödinger equation is solved. According to Mme Février, the predictive characteristics of physical theories divide them into two, mutually exclusive, classes, viz. 'objectivist' and 'subjectivist'. In an objectivist theory, e.g. of classical physics, there exist state variables which allow us to calculate all other quantities. The state variables are simultaneously measurable, in principle; the theories are determinist, etc. We need consider only the state of the system, and the process of measurement or the influence of the observer is of no account. The subjectivist theories do not possess state variables; there exists at least one pair of variables that cannot be measured simultaneously, both in principle and in fact; the theories are indeterminist; we have complementarity, etc.

Another characteristic of theories is their completeness. A theory $Th_1$ is more complete than another $Th_0$ if it contains a parameter which the latter ignores, provided $Th_1$ predicts the same results as $Th_0$ in the domain of adequacy of $Th_0$ when the same initial data are used. This entails that, if two parameters are not, in principle, simultaneously measureable in $Th_0$, they are equally not so measurable in the more complete theory $Th_1$. Thus wave mechanics turns out to be an open or incomplete theory. We can always suppose that it leaves out certain parameters which a more complete theory would include: but such a theory has the same structure as the original one. It is therefore impossible to re-establish determinism by trying to complete quantum mechanics. No microphysical theory can be constructed that is deterministic and of such a kind that all its parameters are in principle (and without restriction) measurable.

Einstein was therefore quite right when he declared that the actual quantum theories are incomplete, in the sense that it is always possible to introduce more parameters: but any more complete theory has the same 'subjectivist' structure. No appeal to 'ignored' parameters can help, for a deterministic theory can never deal adequately with quantum phenomena. Quantum mechanics is an essentially incompletable theory.

This characteristic of quantum mechanics is shown up more clearly when we investigate how quantum phenomena are measured. The author begins the discussion with a philosophical question. Should we be 'realists' and assume *a priori* the existence of the physical world, that is, before we have measured anything? Or should we be 'phenomenalists' accepting as real only what has been observed and measured? This well-known philosophical puzzle has been posed only too often since quantum mechanics began. Mme Février rightly says that so-called realist descriptions make use of 'hidden' parameters which are in principle inaccessible to measurement. In this manner they succeed in describing an 'objective' world in which everything happens according to deterministic laws, independently of observer and of measuring instrument. But such a description, though more complete, merely adds fictitious, i.e. not measurable, quantities: it is pure metaphysics. While recognising that both phenomenalism and realism are philosophical theories, Mme Février feels that phenomenalist interpretations are the lesser of two evils. Her ideal is expressed by Duhem's statement that a scientific theory should be free of all philosophic intent.

Thus the author prefers to oppose a ' predictive ' to a ' realist ' interpretation, because it is restricted to observable phenomena. If we want to calculate future states of a microphysical system, then we have to make use of a measuring instrument; for a microphysical phenomenon cannot be directly observed. The influence of the instrument cannot in principle be eliminated, and so there are always parameters that cannot be simultaneously measured with others. We can make predictions only at the cost of uncertainty. It is impossible to arrive at predictions with the help of a ' hidden' determinism, and so realist descriptions are not refutable by experiment and, therefore, metaphysical.

Mme Février also discusses the idea of a pilot-wave which de Broglie had renounced at the Solvay Congress in 1927 in favour of Born's statistical interpretation. The pilot-wave as well as the theory of the double solution has recently been put forward again by Bohm and Vigier and also by de Broglie himself. But neither proposal is successful in bringing back determinism. There cannot be a functional relation between a physical wave which may guide a particle and the abstract Schrödinger wave which allows us to predict the future state of the particle. The waves are only statistically correlated to one another.

Empirical propositions are about the initial conditions or data from which we start in order to predict the results of later measurements. Or, by means of logic we can derive from the original propositions new ones that are equally concerned with experiment. Such a logical calculus of experimental propositions differs in structure for the two types of physical theory. In an objectivist or classical theory the propositions follow a Boolean algebra. Since a quantum theory contains variables not simultaneously measurable, a proposition such as $p$ & $q$ is excluded and, with it, the possibility of using Boolean algebra. A modal logic may be used instead.

As I have said before, the conclusions reached by Mme Février are not new, e.g. London and Bauer, von Neumann and Birkhoff, or Reichenbach among others more or less arrived at them as well. The arguments presented in this book are, however, more detailed and, though sometimes difficult to follow, logically rigorous and elegant. Anyone who is out of sympathy with the reactionary idea so fashionable today, that is, the idea of a ' hidden ' determinism, will find here strong support, while the fellow traveller will—I hope—change his views if he reads this book.

E. H. Hutten

## *Fact, Fiction, and Forecast.* By N. Goodman.

University of London, The Athlone Press, 1954. Pp. 126.

This short but engaging book is made up of four chapters. Chapter I is a reprint of the article published by Professor Goodman in the *Journal of Philosophy*, 1947, **44**, as ' The Problem of Counterfactual Conditionals '. Chapters II-IV contain three lectures which the author delivered in the University of London in 1953 under the general title *Fact, Fiction, and Forecast.*

The main theme of the book takes origin in the problem of possibility but it is developed along the lines which involve the treatment of dispositional properties, the validity of induction, confirmation theory, and the nature of scientific laws and lawlike hypotheses.

The analysis of counterfactual conditionals occupies the pivotal position in Goodman's enquiry. He believes that a solution to the problem of counterfactuals would give us the answer to a number of baffling questions in which the philosophy of science and the theory of knowledge abound. Now, the problem of counterfactuals, as Goodman sees it, consists in defining the circumstances under which a given counterfactual holds while the opposing conditional with the contradictory consequent fails to hold (p. 14). It is obvious that counterfactual conditionals cannot be interpreted as truth-functional compounds, and Goodman is right in emphasising that the truth-value of a counterfactual depends on a different kind of connection between the two component statements. The difficulty is to find a satisfactory explanation of this connection. In the first place certain circumstances not stated in the antecedent are assumed to occur. Secondly, the connection between the antecedent and the consequent is not a law of logic but what we call a natural, or physical, or causal law (p. 17). Thus we have the problem of ' relevant conditions ' and the problem of ' law '.

With most commendable thoroughness Goodman examines a number of attempts at determining the set of relevant conditions for a given counterfactual only to come to the conclusion that we are not yet in possession of the required criterion. As regards the problem of law, Goodman shows that it involves the question of how to define the circumstances under which a statement is acceptable independently of the determination of any given instance.

The troublesome puzzle of the counterfactual conditional is taken up again in Chapter II. Here Goodman attacks the problem by examining the nature of dispositional statements and dispositional predicates, which apparently present us with less difficulty. His aim is to explain dispositional predicates in terms of manifest predicates without departing from the principle that both types of predicate apply to *actual* things. To put it in other words, the question is how beginning with a manifest predicate like ' burns ' we can *project* it to a wider range of actual things by defining a correlative predicate like ' inflammable '. It is evident from this example that the problem of dispositional predicates and that of projecting manifest predicates to unknown cases is closely bound up with the problem of induction, to which Goodman turns in Chapter III.

The traditional difficulties associated with the logic of induction result from fruitless attempts to justify inductive inferences. Goodman argues that such attempts should be abandoned and that efforts should be made instead to formulate rules which would define the difference between valid and invalid inductive inferences and distinguish between confirmable and non-confirmable hypotheses. The problem of induction conceived in this fashion turns out to be part of the more general problem of *projection.*

The prospects of a theory of projection are discussed in Chapter IV. To begin with Goodman explains what he means by actually projected hypotheses. In accordance with his definition a hypothesis is actually projected if and only if it is adopted and if at the time in question it has some undetermined instances, some positive instances, and no negative instances. A hypothesis which is exhausted, unsupported, or violated does not qualify for the status of an actually projected hypothesis (pp. 91 f.). On the basis of actual projection Goodman proceeds with the preliminaries which in his opinion may lead to a complete definition of projectibility. His initial step amounts to establishing three principles for eliminating unprojectible hypotheses : (i) a projected hypothesis *H* is to be rejected if it conflicts with a non-projected hypothesis *K* which is supported unviolated and unexhausted, and which has a no-less-well *entrenched* antecedent-predicate and a much better entrenched consequent-predicate ; (ii) a projected hypothesis *H* with an ill entrenched consequent-predicate is to be rejected if it conflicts with another hypothesis *K* (1) that has the same antecedent-predicate and a much better entrenched consequent-predicate and (2) that is either (*a*) both supported and violated or (*b*) neither ; (iii) a projected hypothesis *H* is to be rejected if there is another supported unviolated and unexhausted hypothesis *K* with the same consequent-predicate and a better entrenched antecedent-predicate, and if the supporting instances of *H* and *K* are the same but the antecedent-predicate of *H* applies to some things to which the antecedent-predicate of *K* does not apply.

Hypotheses which are supported unviolated and unexhausted, and which are uneffected by the principles of elimination, are called presumptively projectible hypotheses. As a further step in the development of the theory of projection the concept of initial projectibility index for presumptively projectible hypotheses is introduced and used for the purpose of defining degree of projectibility. A complete and final definition of projectibility is not worked out in the course of the present enquiry, which aims rather at illustrating some of the resources that a theory of projectibility may offer.

As a whole the book under review is both original and stimulating. Its chief merit seems to lie in what might be described as the clearing of the ground for a more promising treatment of some old but none the less fascinating problems. The topicality of the subjects discussed and the clarity of exposition are likely to earn the book many a reader from among methodologists, logicians, and philosophers.

Czesław Lejewski

*The Dramatic Universe. Volume I: The Foundations of Natural Philosophy.*
By J. G. Bennett.
Hodder and Stoughton, London, 1956. Pp. 534. 42s.

Mr Bennett is an author who is trying to give an all-embracing view of the cosmos. This introduces difficulties for his reviewer. One difficulty is that Volume I is therefore only half of a book, presenting half of the scheme, and Volume II is not expected to appear soon enough to justify postponing comment until the whole can be embraced. A much greater difficulty is that there are so many policemen about—specialist policemen on point duty, and semanticist policemen pounding the beat. The former insist on orderly movement according to certain rules along certain paths; the latter are likely to turn up anywhere. Their first question to the author ' What little game do you think you're playing? ' will be followed by a demand either for his credentials or for proof concerning the truth of his statements. Most scientists can tell what they think they are doing (if not what language game they are playing), most artists cannot, and Mr Bennett does not quite fit into either class. Consequently it may not be fair even to evaluate his book in terms of what he modestly claims it to be: an attempt to show that it may one day be possible to present a philosophical system embracing all human experience.

This volume is about the processes studied by physics, chemistry, biology, and cosmology; the second volume is to be about values.

After some preliminary chapters on knowledge and language, entities are classified in terms of twelve levels of organisation. The first four cover sub-animate existence: (1) the undifferentiated ground-state, hyle; (2) polarity, invariant being (from the neutrino to the $\mu$-meson); (3) relatedness, identical recurrence (nucleons, etc.); (4) subsistence, composite wholeness (atoms and molecules). After a transitional level (active surface, colloids) there are four levels of animate existence (viruses, cells, metazoa, species). Then a second transitional level (the biosphere) is followed by four levels of ' supra-animate ' existence (planets, solar systems, galaxies, the knowable universe).

For describing process, four ' determining conditions ', forming a six-dimensional framework, are used: space, time, eternity, and hyparxis. Space and time are the dimensions of the actual. Eternity is the dimension of potentiality, relevant to probability. Hyparxis is the measure of ableness-to-be, recurrence in time; the hyparchic interval is said to vanish when recurrences of an entity are identical, and would thus seem to be a measure of development and decay on a given level of organisation.

One recognises the things that put the policemen on the alert—mention of the biosphere and eternity—and two warnings are enough. It had better be said at once that this book has nothing to offer those to whom ' speculative ' is invariably a derogatory adjective or those who cannot enjoy an hypothesis before they have decided whether it is falsifiable or confirmable; it will be of no more use to them than is a handful of fertiliser to a hungry man, or a gymnasium to a sprinter in the course of a race. I suspect that this leaves a class of readers that at present is rather small.

It is concerning the first four levels of organisation that Mr Bennett really seems to have something to say. It is on these levels that the six-dimensional geometry appears to be invested with some quantitative meaning in relation to motion, energy, action, the mode of existence of elementary particles and their modes of interaction. The discussion is detailed, often ingenious.

The concept of a time-like dimension along which probabilities may be measured is attractive—but of course in the absence of any generally acceptable way of talking about probability, which seems to bring out the worst in the best of us, one's sales resistance to a new concept is rather low. The concept of the time-like dimension of hyparxis is difficult to grasp, but it may possibly provide a useful way of talking about certain types of process.

Concerning the other levels of organisation, Mr Bennett is relatively perfunctory. His levels of animate existence are the familiar ones. Here, the two time-like dimensions seem less relevant—but not entirely irrelevant. For example:

> Sexipotent entities [cells] have no variation inherent in their eternal pattern and their potentialities must therefore be worked out through multiplication and recurrence. The sensitive hyparchic regulator has a space-like component, directed towards the environment, and it has also an eternal component, directed towards the common pattern. It thus provides a linkage between unity and multiplicity that is both flexible and restrictive. The restrictive character of the organic sensitivity in a single cell is due to the absence of mutual regulation between different levels of organisation.

The first three sentences appear to apply to the nucleus rather than to the cell; otherwise the implication is that cell differentiation occurs by jumps when cells divide, each jump being regarded as occurring after the independent life of the parent cell has ceased and before the independent lives of the new cells have commenced. The last sentence is quite unsatisfactory, unless I have misunderstood it: cells do regulate organisms, and vice versa.

At the top of the animate levels, the author puts the biosphere, a disreputable concept. If this is regarded simply as the domain of ecological relations, there is nothing to get bothered about ; it is the first reference to its *rôle* that makes one feel uncomfortable. Photo-synthesis, yes ; oxygen circulation, yes ; but ' The Biosphere exists to transform energies required for the maintenance of some form of hypernomic existence probably within the solar system ' gives one the feeling that Mr Bennett is here closing his eyes, holding his nose, and jumping into a language he has hitherto succeeded in avoiding. The brief section he devotes to ' the cosmic order ', his last four ' supra-animate ' levels, appears to be no more than a superficial account of the descriptive matter in any elementary textbook of astronomy, with his systematic terminology just stuck on. There seems to be nothing here that might suggest new concepts of process or relation. However drawn emotionally one may be towards any aesthetically satisfying principle of cosmology, there appears to be nothing here that cannot be talked about in the languages of the author's four lowest levels—with the possible exception of gravitation. After all, there is the simple fact that the emergence of what we call life came after the formation of the galaxies and after a lot of geological process as well.

It should be mentioned that few of the names in the index are not considered eminently reputable. Aristotle, Kant, Poincaré, Minkowski, Einstein, Whitehead (to whom I think the book owes most) and Eddington are among the most frequently referred to. However, the fact that there is reference also to Gurdjieff calls for a comment that is perhaps of wider relevance.

Gurdjieff practised a system of psychotherapy which, one may believe, has given relief or satisfaction, directly or indirectly, to several people: i.e., in certain circumstances, it works. The system's verbal form is allegory or myth, embracing the universe; its effect, like that of poetry or liturgy, is to work on people, to alter states of mind. Its language game is different from those used for other purposes, such as making predictions about the world of physics. It would appear, however, that Mr Bennett, Ouspensky, and others, impressed by the effect Gurdjieff's words had on their states of mind, proceeded to confuse anagogical efficacy with literal truth and to regard these words as expressing propositions about the world of physics and biology. It is interesting that they should have done so, as it seems unlikely that they would have regarded, say, the koans of Zen in the same way.

It seems fair therefore to regard Mr Bennett's introductory acknowledgment to Gurdjieff as equivalent to those of the form ' To . . ., without whose encouragement and support this book would never have been completed '. At very least, the book provides an advanced exercise in semantic analysis, and some readers may find it valuably provocative.

G. E. Denyer

*The Direction of Time.* By Hans Reichenbach.
University of California Press, Berkeley and Los Angeles, 1956. Pp. vii + 280. £2 1s. 6d.

A PURELY mechanistic universe consisting of mass points which obey strictly deterministic laws presents various difficulties to the understanding, two of which are closely connected with the topic of the book under review. The first difficulty is that such a universe apparently does not provide any *physical* reason for the existence of a unidirected time. The second difficulty is that it apparently does not allow us to give an account of laws involving direction, such as the second law of thermodynamics. Both difficulties were solved, over fifty years ago, by Boltzmann's epoch-making investigations. The solution which Boltzmann proposes is very simple. According to Boltzmann the *second law* of thermodynamics is only statistically valid: given a class of closed physical systems, all of them in the same initial state of low entropy, the great majority of those systems will eventually assume a state of higher entropy, and this in spite of the fact that a *single system* behaves in a perfectly reversible way. The *direction of time* in a given part of the universe is determined by this behaviour of the majority and it is therefore a property which possesses neither spatial, nor temporal universality. (It may change from one epoch to the next, and it may also change from one part of the universe to the next.) Reichenbach's essay which is mainly concerned with what we above called the first difficulty may be regarded as an exposition, enriched and enlivened by many more or less relevant asides, of this solution of Boltzmann's (which Boltzmann himself managed to condense into two pages of vol. ii of his *Gastheorie*).

Its central part, i.e. the proof that time direction is a property not of a single system, but of an *ensemble* of systems with a common initial entropy, is based upon five assumptions which Reichenbach calls the *hypothesis of the branch structure* and which may be regarded as a slight clarification of Boltzmann's own presuppositions. It is interesting to note that three of these assumptions (3, 4, 5 on p. 136) have been proved to follow from classical mechanics (the proof is due to the work of Birkhoff and his school). In particular assumption 5 (' in the vast majority of branch systems the directions towards higher entropy are parallel to one another, and to that of the main system ') can be shown to be a highly probable consequence of the laws of classical mechanics (it is valid for ' almost all ' points of the trajectory of the ' universe '). This suggests that by replacing assumption 1 (' the entropy of the universe is at present low ') with the assumption that the universe is dwelling in a state in which assumption 5 is strictly valid it would be possible to define a time direction for the whole of the universe. That such a procedure should be possible, at least for some stages of the development of the universe, is also supported by the fact, first made known by O. Heckmann, that a classical universe is unstable and must expand, or contract, or pulsate just as its relativistic colleagues.

Whereas Boltzmann was dealing with a mechanistic universe, Reichenbach assumes ' that the quantum physics of today is in need of Boltzmann's ideas just as much as the physics based upon Newton's mechanics for the very reason that this modern physics, too, did not discover irreversibility in its elementary processes ' (134). This assertion touches a sore point of the interpretation of quantum theory. For few physicists would be prepared to admit that the continuous reversible changes of state implicit in Schrödinger's equation, are the only elementary processes of quantum mechanics. Both Bohr and von Neumann have asserted a dualism of reversible and irreversible processes, and Landé has even gone so far as altogether to deny that Schrödinger's equation may be regarded as a process equation. On the other hand it seems that attempts, such as Ludwig's, to explain the ' quantum jumps ' as macroscopic phenomena which microscopically can be understood on the basis of Schrödinger's equation alone are either open to the reversibility objection, or employ a postulate which is equivalent with the assumption of irreversibility upon the macrolevel. This being the case the extension of Boltzmann's procedure to the domain of quantum mechanics seems certainly to be advisable.

The exposition of Boltzmann's theory is only part of the content of Reichenbach's essay. The remainder deals with such diverse topics as the emotive significance of time, entropy and information, quantum theory, the Feynman tracks and others. There are excellent suggestions (cf. the treatment of the Gibbs paradox on p. 63), long stretches of more or less efficient popularisation, repetitions of arguments and results which are well-known from other works of Reichenbach's. On reading the book one frequently (but not always) finds oneself confronted with the rather perplexing question as to whether that new discipline, called ' philosophy of science ' is really as new as it looks, or whether it is only the old enterprise of popularisation made new and fashionable by the use, in addition to the technical terms of physics, of the technical terms of the one or another ' scientific ' philosophy. But this question leads already far beyond the book under review, for it concerns nearly the whole of the present ' philosophy of science '. The fact that it does not apply to the whole of *Direction of Time* recommends this book both to scientists and to philosophers.

P. K. FEYERABEND

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*Experiment and Theory in Physics.* By Max Born.
Dover Publications, New York, 1956. Pp. 44. 60c.

THIS short essay contains the substance of an address which the author delivered in 1943 to philosophers and scientists. In it Professor Born puts forward some of his reflections on the nature of scientific inquiry taking as his starting point, on the one hand the extreme views of some distinguished experimentalists such as Lenard and Stark who reject all theory (as an invention of the Jews !) and on the other hand the views of some distinguished theoretical physicists, in particular Milne and Eddington, who held that the laws of nature could be discovered by epistemological and mathematical arguments without appeal to experiment.

The author's own views on the relation between experiment and theory are expressed mainly by means of illustrations from the history of physics, in particular the physics of the nineteenth and twentieth centuries, much of which has actually been given its present shape by Born himself. The highly exaggerated claims of the defenders of ' pure experiment without theory '—an expression which to most philosophers and physicists appears to be a contradiction in terms—are deflated by mentioning, among other things, Maxwell's most famous mathematical guess. This consisted in adding without proper experimental reasons, the term $\frac{1}{c}\frac{\partial E}{\partial t}$ to the right-hand side of the equation curl $H = 0$—a modification which as Born emphasises leads to the electro-magnetic theory of light and to many other theoretical and experimental discoveries. Mathematical guessing of the kind described has nothing to do, however, with *a priori* reasoning; it is rather the work of an intellectual instinct in those who know the facts of experience and for whom mathematics is the natural language of physical inquiry.

The exaggerated claims of the *a priori* school of physics are attacked *via* a brief analysis of some of the works of Eddington and Milne. It is suggested that there are no good reasons for choosing on *a priori* grounds between these two *a priori* theories, even where their results are in a perfectly straightforward sense contradictory.

Born's own conception of the nature of a physical theory is very briefly explained by reference to the psychological *Gestalt*-theories. As these explain apprehension of shape in physical things, he wishes to explain apprehension of the invariants of physical theory and physical reality. Seeing a man behind a desk is not ' seeing coloured specks ' and then ' composing them by a secondary process '. The creative physicist similarly sees a *Gestalt* ; and he too may be right or wrong ' about the real shape of a partly known phenomenon '. If confirmed by experiment the physicist's apprehension produces new knowledge. But the ability to apprehend it rightly ' depends in a high degree on intuition and can hardly be learnt '.

The philosophical—and antiphilosophical—*obiter dicta* of physicists about their subject are often quite unenlightening to both philosophers and physicists, but Professor Born's lecture certainly does not belong to this type of literature. It is on the contrary highly interesting, informative, and in every way worthy of the attention of philosophers and, I should think, of physicists too.

S. Körner

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*Descartes's Rules for the Direction of the Mind.*
By Harold H. Joachim (Edit. Errol E. Harris).
George Allen & Unwin, 1957. Pp. 124. 10s. 6d.

The late Professor Joachim's lectures on Descartes's *Rules* were originally delivered in the early 1930's as a supplement to the lectures later published as his *Logical Studies.* The manuscript of the Descartes lectures was, however, subsequently lost, and the volume under review consists of a reconstitution of these from the notes taken by the editor and by Professor John Austin when they were Joachim's pupils. It was obviously a labour of love, and it should be said at the outset that the work has been admirably done. This is the authentic voice of Joachim, his ' close and luminous ' style, as it has been called. All those who have read Joachim's books, or who attended his lectures, would, I am sure, agree.

Whether, in the present philosophical climate, they would be able to accept Joachim's conclusions, is another matter. The purpose of the lectures seems to have been twofold. Joachim was, of course, concerned in the first place with giving a faithful, though critical, exposition of Descartes's view of how the reasoning process is properly directed; but he seems to have been equally concerned with confronting the Cartesian view with his own theory, usually known as the coherence theory of truth. The full force of this comparison and contrast is reserved for the last lecture, entitled the ' Philosophical Analysis of the Concrete '—a title which sufficiently indicates Joachim's line of criticism; but the double intention underlies the first three lectures as well, and tends to give Joachim's objections to the Cartesian view an air of prejudice.

The *Rules* were begun most probably soon after Descartes's return to Holland in the year 1628. The years of apprenticeship, of which he speaks in the Discourse, were at an end, and Descartes was prepared now to embark upon the mission that had been revealed to him on the famous night of 10th October 1619. The first nine months were spent, he tells us in a letter a few years later to Mersenne, in the composition of a little treatise on divinity—a first sketch, it may be supposed, of the subsequent *Meditations.* But the task on which Descartes had set his heart was the elaboration of a complete physical theory of the universe. This work, which Descartes jestingly described as his ' Roman du Monde ' was not destined to be published. The condemnation of Galileo for maintaining the movement of the earth, which was an integral part of Descartes's own conception of the universe, was enough to make him suspend publication of the work as a whole, and to let his views infiltrate, as it were, through two of the three treatises prefaced by the *Discourse*. The *Rules* were an attempt to codify the modes of reasoning which Descartes was putting into practice at the time. They reveal Descartes's constant preoccupation—the need to harmonise his physics and his metaphysics. It is a point to which Joachim paid no attention, any more than he seems to have been prepared to allow fully for the tentative and fragmentary character of the *Rules*. For that is what they are—tentative and fragmentary—and it is almost too easy to take note, as Joachim did in these lectures, of the many inconsistencies and unsafe conclusions. True, Descartes intended to write a book, of which the plan may be discerned in the *Rules*; but if the *Rules* may be regarded as a first sketch for the *Discourse* a sort of ' ballon d'essai ', it was a balloon that never went up. Descartes left the *Rules* unfinished and unrevised except that he reduced them to four in the *Discourse*. The manuscript itself, as the editor shows, survived almost by chance.

This is not to say that the *Rules* may be neglected by students of Descartes. They are of great historical interest for the understanding of Descartes's finished works, the *Meditations* and the *Principles* as well as the *Discourse*, and it is a great pity that Joachim neglected this aspect of the work in favour of enforcing the contrast between his own theory and that of Descartes. The main charge brought against Descartes in these lectures is that his view of reason makes it largely an instrument of analysis and so incapable of grasping ' wholes '. But even wholeness is an aspect of the object which we perceive by analysis. In fact, we must take our reason, as T. S. Eliot once counselled us to take contemporary poetry, as we find it. We may describe it, if we please, as the ' false dissecting power ', as long as we are aware that hard words break no bones. Even now, separated though we are by more than three centuries from the first formulation of the *cogito* in the *Discourse*, we can still, I think, understand Descartes's conception of the movement of the mind in the search for truth. The process begins with an initial intuition from which other intuitions flow by a rigorous deduction. Then comes an immense effort to transcend deduction itself by reintegrating its successive stages in a comprehensive vision of the whole body of human knowledge grounded on the evidence of its first truth and sharing in its light. An unattainable ideal, no doubt, and perhaps a misleading one, but far removed from the sort of rationalism which Joachim appeared to find in Descartes.

ARTHUR WOLLASTON

## ANNUAL CONFERENCE OF THE PHILOSOPHY OF SCIENCE GROUP

THE third annual conference of the Philosophy of Science Group of the British Society for the History of Science was held from the 19th to the 21st September 1958 at St Hilda's College, Oxford, under the Chairmanship of Professor R. O. Kapp, chairman of the group, and was attended by about sixty members and guests. Dr M. B. Hesse was conference secretary.

Symposia were held on ' Simplicity in Scientific Theory ', ' The Scientific Status of Economics ', ' Other People's Concepts : Interpretations in Anthropology ' and ' Theoretical Concepts '.

In the first symposium, papers were read by Dr H. R. Post and Dr Mary B. Hesse, and Professor R. O. Kapp took the chair. Regarding simplicity as a third requisite of a theory, alongside logical consistency and correspondence with experience, Dr Post suggested a case for attempting to define this quality, noting that it could be shifted between concepts and relations : any theory could be expressed in the form $X = 0$, at the expense of complicating $X$. Three fields of simplicity were in fact distinguishable : the linguistic, the semantic and the pragmatic (suggestive power). Dr Hesse did not regard simplicity as valuable in itself as a linguistic or semantic characteristic of a theory, but only as a pragmatic characteristic, namely that it makes for ease of apprehension. Linguistic simplicity, depending on choice of technical terms or of co-ordinate system, was irrelevant to the *content* of a theory. The discussion revealed two shortcomings of terminology: the term ' theory ' is still applied rather indiscriminately to many types of statement including uniquely specific inference, generalised description, deductive system, and even to observational description ; and the term ' simple ' suggests ' easy ' to some and ' economical ' to others. After consideration of examples afforded by atomic, evolutionary, relativity and quantum theory, however, the session ended with at least a shared intuition, if not with a definition, of one sense in which the term ' simplicity ' might be used in speaking of a scientific theory.

Mr W. C. Kneale took the chair in the second symposium. Professor T. W. Hutchison outlined the controversy among economists on whether it is possible to identify and to separate judgments of value from judgments of fact in integrating the latter into a system meriting the adjective ' scientific '. He argued that ' is ' and ' ought ' statements are always separable in principle, and that the attempt must be made to separate them in developing a disciplined universe of discourse. Dr A.W. Stonier accepted the logical distinction but argued that, in economic *research*, the raw data (especially verbal behaviour), however factual they may appear, tend to have human evaluation built-in ; it might be possible to identify and to separate valuation components in the course of an investigation, but it seemed hopeless to try and choose only factual data at the outset. Several speakers referred to situations where fact and value judgment *are* clearly separable, extending the discussion to include other social studies and operations research (in which value judgments are controlled by being made explicit). The view was also expressed that perhaps a notion of ' Science ' that excluded from its legitimate field any concept of the motivation of human behaviour was unduly restrictive.

Mr J. W. N. Watkins took the chair for the third session. Mr E. A. Gellner considered a problem of anthropological method : what should be the attitude of the investigator of an exotic culture to verbal statements that do not appear at first to ' make sense '? The assumption of ' primitive mentality ', of a radically different (and ' inferior ') mode of thinking, has gone out of fashion. Current techniques of interpretation assume that sense, *good* sense, is to be made of any statement provided sufficient account is taken of behavioural consequences, of the context in which the statement was made, or of idiosyncrasy of the speaker. Unless used with caution, however, these useful techniques may invite false conclusions : they can be used to make sense of any utterance whatever, and they exclude certain types of interpretation known to be valid, as where a statement may be intended to influence behaviour by virtue of its very absurdity. Mr E. R. Leach accepted Mr Gellner's scruples, but considered that the safeguard lay in developing a wider view of possible contextual relations (including relations with the language as a whole, as in punning) rather than in rejecting the contextual approach. The conference came to grips with this problem by proceeding rapidly to generalise it to embrace the Freudian analysis of the *lapsus*, the distinction between expository and emotive uses of language, the peculiarities of abusive and amatory word-play, and the difficulties of conceptual analysis and inter-disciplinary communication within one culture. Most speakers thought that there was little danger in the assumption that nearly all phenomena, including noise, are explicable in some sense, provided this is not held to entail that every phenomenon is worth explaining or that any statement (e.g. in a political doctrine) is *ipso facto* valid.

Dr A. C. Crombie took the chair for the fourth session. Mr G. Buchdahl made a case for distinguishing two types or levels of scientific discourse, the phenomenological and the epistemological, about theoretical entities such as atoms. He drew on the history of atomic theory in the first half of the nineteenth century for examples of how confusion of these two levels can lead to theoretical impasse: given certain phenomenological relations, there were phenomenological and epistemological questions about what unobservable things could have properties that would yield these observable relations. Dr J. Agassi held that there are meaningful connections between some views concerning the possibility of theoretical knowledge, some views concerning reality and some views concerning the method of learning. Discussion was vigorous, turning largely on whether using a concept that refers to a hypothetical entity entails any assumption about existence, and on the validity of attempts to progress from ' These phenomena occur *as if there existed* such and such entities having such and such properties ' to ' These phenomena occur *because there exist* such and such entities, etc.'.

The conference closed with a general discussion of possible subjects for a fourth annual conference to be held in September 1959.

The liveliness and quality of discussion throughout the conference, and the success shown by workers from many different fields in ' tuning in ' on one another's modes of thought, provided welcome assurance that the ' fragmentation of science ' is not everywhere accepted as a necessary evil. The informality of the proceedings, moreover, permitted the rapid and fruitful exchange of nascent ideas, in which the historian, the philosopher, and the logician could mediate when any purposes threatened to become crossed.

G. E. Denyer

# ABSTRACTS

## *Dialectica*, 1957, **11**, Nos. 1/2

C. Perelman, 'Evidence et Preuve'

It is shown that a theory of proof which repudiates the classical conception of the obvious can nevertheless be elaborated, thanks to the elements provided by the analysis of reasoning, the study of which, unlike the study of demonstration, has for centuries been neglected.

W. Pauli, 'Phänomen und physikalische Realität'

Without accepting or favouring a particular philosophical '-ism', 'phenomenon' and 'reality' are analysed from the standpoint of the professional everyday life of physicists. The logical structures of physical theories, including its characteristic connections with observation and experimentation, is briefly indicated, with classical mechanics, classical relativistic field theories and quantum mechanics as examples. It is emphasised that physicists consider their science as being in the course of a development. The problem is therefore not whether or not the present theories will remain as they are, but merely in which direction will they change?

J. Rossel, 'Caractéristiques, tendances et implications de la recherche atomique actuelle'

The present situation of scientific activity, especially in atomic physics, has led to an appreciable alteration of the traditional spirit in basic research. The main reason for this modification appears to be the increasing bond between science and technology. As a consequence of this deep and intricate relationship, the influence of the political state, and of large financial organisations upon fundamental scientific work, has developed to a degree as yet unknown.

On the other hand, the result has been an industrialisation of basic research. From this state of affairs arises a limitation in the freedom of research and information, and often for the physicist a questionable propensity to megalomania.

The need for a study involving both science and philosophy is necessary when considering the implication of atomic research.

J.-L. Destouches, 'Aspect dialectique de la notion de système physique'

The essential part played in physics by the notion of system is discussed. We investigate here the latest *functional theory of particles* which consist in using for the representation of a particle a more complete picture than those which were used till now, the picture of a function *u* of a functional separable space.

H. König, 'Zur Analyse des Grössenbegriffs'

The author shows that many difficulties disappear when two entirely different axiomatic bases are accepted from which two entirely different manners of calculating with quantities and thinking in quantities are derived.

R. P. Dubarle, 'Formalisations et théorèmes critiques'

The writer aims at disengaging the philosophical significance of various mathematical theorems dealing with formalisations. These theorems form the part of epistemology which may be expounded in the technical form proper to that branch of science and bring together the elements of a self-critical reflection of mathematical thought. They also suggest that the act of mathematical thought should be understood as the unfurling of a thinking process implying simultaneously a deliberate initiative of thought, an intuitive field altogether indefinite and a logical normative requirement. In conclusion, they induce the philosophy of science to wonder whether or not mathematics as they stand are the only possible form of synthesis—in the strict sense of the term—between these three moments or factors of thought.

G. Bouligand, ' L'Activité mathématique et son dualisme '

A comprehensive study of mathematical thought, taking the actual work for its basis, will examine mathematical activity first. Such activity has always set itself mutually dependent aims of two sorts:

The solving of problems, i.e. determining some unknown element under precise conditions: and the achieving of a synthesis which, in the presence of new problems, simplifies and, if necessary, co-ordinates anew the known facts of whatever sort (methods, operational potentialities . . .).

This is what allows us to draw up a specific scheme regarding the historical evolution of deductive science, the dialectical aspect of that evolution has been stressed as far as possible. It brings in Logic in a natural way, and shows clearly how ' mathematisation ' operates in a wide field.

H. Guggenheimer, ' Dialektische Synthese von Formalismus und Intuitionismus '

It is show first that the cardinal and ordinal hierarchies of the theory of sets may be interpreted in a finitist, operationalist manner, without renouncing any of its fundamental features. This point of view has remarkable advantages as compared with a strictly axiomatic-formalistic interpretation. A way is indicated to overcome the problem of the inaccessible numbers.

D. Dugué, ' Quelques remarques sur le caractère provisoire de toute axiomatique '

Axiomatisation is given in this paper as the definition of a mathematical system with reference to all possible extensions. The set of these extensions may be infinite and so it is not impossible that the basis of any mathematical system is an infinite set of axioms.

This point of view is illustrated by examples from geometry. The author gives a statement of topology which he considers as an axiom boundary between certain mathematics and stochastic mathematics.

J. Ruesch, ' Principles of Human Communication '

Psychopathology and social pathology can be conceived as disturbed communication and the various methods of therapy, both physical and psychological, are geared to improving the organs and functions of communication in man.

Disturbed communication is considered as a special case of ordinary communication that is distorted through erroneous timing, deviations in intensity, and inappropriateness of messages.

M. Monnier, 'Plan d'organisation, continuité et stades du développement des activités électriques cérébrales'

The analysis of the development of the electrical activity of the brain in children, with EEGs recorded in longitudinal series allows us to express certain general remarks on the organisation, continuity, and discontinuity (levels) of the brain development. The method is both objective and subjective.

The analysis of longitudinal series of EEGs shows that the electrical brain activity develops in a *continuous*, progressive way, according to an organisation plan (Bauplan). This plan is partially expressed in each EEG, but completed only in the definitely organised pattern of the 15 years old adolescent. In this continuity of the development, however, there appear certain periods when the organisation of the electrographic pattern seems to be intensified, accelerated, consolidated. These 'epochs', or levels, appear at 6 to 7 years, 9 to 9·6, 11, 13, and 15 years. It is possible to detect a certain correspondence between this conception of the development and that of Piaget and Inhelder. The chronology of the electro-physiological levels corresponds to that of the cognitive functions.

C. J. Herrick, 'Analytic and Integrative Nervous Functions'

Analytic functions, typified by reflexes, use stable inherited structures which are organised for adjustments in behavioural space and time. Individually acquired integrative functions, typified by conditioning, symbolism, and reasoning, are wholly internal and may use any nervous tissue not differentiated for other specific activities. They must be defined relativistically. The highest members of both classes are in the cerebral cortex, the first in the relatively stable projection centres, the second in the more labile areas of elaboration where there is no inflexible localisation of function. When consciousness emerges within the second class, the mechanism employed probably must be described in accordance with still unknown relativistic principles.

Charlotte Bühler, 'About the Goal-Structure of Human Life'

The present study is to survey some of the concepts and theoretical considerations to be applied in the study of individual's goal-setting, toward a general theory of goal-setting in human life.

W. Servais, 'Pédagogie mathématique ouverte'

As a philosophy, idoneism is most closely related to mathematics and its methods could and should exercise a deep influence over the teaching of mathematics.

Teaching should be an active dialectic of what is intuition, empirical, and rational. It should rely on concrete instances and progressively organise deductive inference, seeing to it that the requisite mental structures are acquired. Such elements would then mutually integrate, since the mathematical model would be disengaged, inferred, from the intuitive model.

## *Dialectica*, 1957, **11**, Nos. 3/4

P. Bernays, 'Von der Syntax der Sprache zur Philosophie der Wissenschaften'

Newer trends of development in the schools of logical empirism are considered. It is shown how, by the correction of the original too simplifying theses of the Vienna school, one becomes reduced to the study of the traditional problems of the philosophy of science.

A. C. Blanc, 'De l'emploi inadéquat du terme "primitif". Considérations sur l'évolution et la systématique'

The inadequate use of the term 'primitive' is due to a confusion between archaic primitivity proper, and secondary 'primitivity' or pseudo-primitivity. In any kind of evolution, the transitions from simplicity to complexity and from indistinctness to distinctness follow strictly similar courses. Three phases are recognisable: a pre-apogean phase of slow enrichment, an apogean phase of original polymorphism, and a post-apogean phase of segregation and specialisation. It is necessary to distinguish between:

1. the archaic simplicity of truly primitive forms and secondary simplicity of impoverished and specialised forms;
2. original polymorphism and secondary polymorphism due to hybridisation.

A. Mercier,' Science et philosophie'

Science and philosophy are distinct, but they rely upon each other. Both are precarious, in different degrees; both are open.

There are two extreme ontological positions: A kind of scientism consisting in a radical positivism (there are no beings but limited beings), and the transcendental ontological position (the notion of being in the singular is meaningful as infinite source of all existence and as guarantee of the fact that there always will remain work to be done).

A dilemma arises: Neither of these positions can be proved to be the right one. Yet without the dilemma, it is doubtful whether any philosophical problem would arise at all.

There are strong reasons to think that the radical positivistic position is nearer to a closure, whereas the transcendental one warrants the openness of science as well as of philosophy.

R. Bayer, 'L'Evolution de l'intelligence et les formes modernes de la dialectique'

There are two prospects of intellectual evolution in the notion of dialectics: intelligence can either be the ultimate point of biological adaptation or it can already be the expression of reason. Within these open characteristics of scientific dialectics are to be found the interpretations studied here: Baldwin's pancalism, Binet's 'thought without image', Janet's and Piaget's interpretations which help to enrich the knowledge of the genesis of intelligence and its evolution.

L. Rougier, 'L'Evolution du concept de raison dans la pensée occidentale'

The word 'reason' acquired a meaning with the creation of deductive geometry by the Greeks of the fifth century B.C. These geometers substituted for the sensible evidence, which only establishes the 'what' of an observed fact, the intelligible evidence which explains the 'why' of such observed fact, by showing that it is the necessary result of a small number of statements, accepted as self-evident. Rising from the concrete to the abstract, the geometers isolated the intelligible essence from the sensible accident, and substituted for the real the possible. The empirical technics of the Orientals made place for the theoretical sciences of the Greeks: astronomy, mechanics, optics, and musical theory. Instead, however, of seeing mathematical concepts as creations of the mind, the Greek geometers considered than as pre-existing entities, discovered by the eyes of the soul. They furthermore disregarded the practical applications which alone can stimulate experimental sciences. There were the limitations and the causes of the eventual end of hellenic science.

The scholastics sought to reconcile reason with faith, by defining reason as the legacy of Greek science and philosophy, identified, after the thirteenth century, with the Aristotelian Encyclopedia. They failed in this endeavour. The doctrine of the ' double truth ' was the admission of this failure.

In order to progress beyond the scholastic outlook, it was necessary to reject the precepts of Aristotle in the study of natural phenomena, to become free of the tyranny of theology, to develop a correct theory of knowledge, especially as regards the rôle of abstract ideas in the distinction between formal and empirical truths. It was necessary to interpret as a creation of the mind what the Greeks considered the discovery of pre-existing truths. The breakdown of the *synthetic a priori* of the Cartesians and the Kantians leads one to wonder what remains of the *a priori* requisites of reason. The last part of the article suggests an answer to this question.

C. Paris, ' Programme et position historique d'un rationalisme humaniste '

The critical attitude taken towards the idea of reason supplies the starting point; it is considered possible to overcome the resulting crisis by positing this idea anew, in order to establish a new rationalism which might be termed ' humanist '. That rationalism should first of all duly record the fact that our knowledge is strictly human and differs as such from an absolutely intellectual knowledge, in a sense which contrasts with the present rationalist outlook. The writer stresses the ' compositive ' and ' divisive ' aspects as well as the dynamic character of our rational activity, in the process of presenting the explicative possibilities of this dynamic understanding with reference to mathematics and natural science. Kantism is examined in its historical and programmatical position, stressing how deep the rationalist imprint is there, making it a blind alley as far as scientific dynamics are concerned. A way of overcoming this difficulty is discussed.

K. Reidemeister, ' Spekulation und Vernunft '

A theory of knowledge (relating, e.g. to the exact sciences) can only exist as an independent philosophical discipline if we can establish a concept of scientific thought independently of an inquiry into individual sciences. This is the problem, which Wittgenstein tried to solve in his *Tractatus Logico Philosophicus*, not without touching on some very speculative and unknowable things. For that reason, we cannot understand the significance of the *Tractatus* without the idea of speculation. It is an essential and reasonable problem for the theory of knowledge to determine the nature and the limits of speculation. It is a relevant problem also for the theory of science, since physics and speculation have points of contact and some distinguished physicists have introduced speculative ideas uncritically into scientific exposition.

E. Dupréel, ' Consistance et valeurs '

The *degree of consistence* of an object results from its possibilities of resisting external attacks.

There are: the consistency of *beings*, the consistency of *notions* and the consistency of *values*. The most fundamental problems are those concerned with *hierarchy* according to consistence, and above all those concerned with *promotion*, the passing to a higher grade (matter—life—knowledge—spirituality).

K. R. Popper, ' Probability, Magic, or Knowledge out of Ignorance '

A formal axiomatic calculus can be constructed comprising all the well-known laws of probability theory. This calculus can be interpreted in various ways. The present paper is *a criticism of the subjective interpretation;* that is to say, of any interpretation which assumes that probability expresses *degrees of incomplete knowledge*. It is shown in the paper that the subjective theory cannot lead to results which are compatible with the objective theory. This is due to various reasons, the most important of which is that the objective theory interprets ' $b$ ' in ' $p(a, b)$ ' as a statement of the *objective conditions* of an experiment and ' $a$ ' as one of its possible results. The subjective theory on the other hand interprets ' $b$ ' as our total relevant knowledge which will in general include some knowledge of previous results of the experiment. It is shown that this must lead to incompatibility owing to the fact that this knowledge of previous results must influence the value of the probability.

It is shown, in this way, that any probabilistic theory of the process of learning from experience—that is to say, any probabilistic theory of induction—must lead to contradictions.

F. Moch, ' Réflexions sur les probabilités '

Probability shows paradoxical characteristics in classical physics; although dependent upon a state of knowledge, and yielding information only as the total result of numerous trials, it seems nevertheless to be definable as an objective characteristic of the isolated event. When used by micro-physics, it breaks the unity of the physico-mathematical foundations which became apparent in bivalent logic, in its conception as a theory of any object, any event or any theory.

Analysis of these paradoxical characteristics and of the concepts ' object ' and ' event ' shows physics and mathematics regain their fundamental unity if bivalent logic is replaced by the trivalent logic of synthesis (*Dialectica* 35/36) or the logic of attitudes, which generalises if (*Dialectica* 39).

P. Nolfi, ' Spieltheorie und Willensfreiheit '

The present paper shows how the most important problem of ' freedom of will ' can be interpreted and clarified within the framework of the theory of games.

S. Gagnebin, ' Structure et substructure de la géométrie '

The author endeavours to set forth the significance and importance of two writers on geometry. G. Bouligand brings out the algebraic and logical structure of that science and presents an ' axiomatic ' introducing both the notions of ' whole ' and ' group of transformation '. A progressive classification of problems is thus elaborated according to the kinds of solution that suit them. F. Gonseth, in his far more comprehensive work, analyses the intuitive, experimental and theoretical aspects of what used to constitute the elementary in classical treatises, then states the researches raised up by Euclid's postulate and leading to the development of the axiomatic method. Thanks to the introduction of such ideas as ' schematic correspondence ' and ' model ', geometry appears as a rational science in progress using the ' axiomatic ' as its criterion.

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## ERRATUM

In C. Lejewski's review of *Meaning and Necessity* by Rudolf Carnap, No. 35, November 1958:

p. 251, l. 18, *for* ‘Pegasus, is large’ *read* ‘Pegasus’, ‘is large’